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ON THE ARTIFICIAL HATCHING OF MARINE FOOD-FISHES.

THE artificial hatching of freshwater fishes, which was known to the Egyptians and Romans as well as to the Chinese and Japanese, has long been successfully practised, especially since Jacobi of Hohenhausen, in Germany (1748), and Remy, in France (1842), showed that it could be made profitable. Before the latter date, however, *viz.*, in 1837, John Shaw, of Drumlanrig, in our own country, had made important observations in connection with the eggs and young of the salmon. By-and-by the French, stimulated by M. Coste, who had the countenance of the Emperor, made remarkable strides in this department, so that the fine establishment at Hüningue, in Alsace, was soon equipped. The report of the French operations—especially that sent to Perthshire by Lord Gray—attracted the attention of those who took an interest in fish-culture, and various efforts—for example, those of Mr. Ashworth at the fishings of Galway—were made in this direction. The most notable, however, was the establishment, by the proprietors, of the fishings of artificial salmon-hatching at Stormontfield, on the Tay, in 1853. This well-known station proved of great service in giving opportunities for obtaining an accurate knowledge of the development of the salmon and its life-history, as well as in

promoting the prosperity of the salmon-fishings.[1] Since that date the hatching of freshwater fishes has been extensively carried out in the United States, Canada, France, Germany, Austria, Russia, Italy, Switzerland, Greece, China, Japan, Australia, New Zealand, and elsewhere, while in our own country it has also been largely developed by private enterprise, such establishments as those of Sir J. G. Maitland, at Howieton, being both extensive and successful. The British Government, however, unlike those of the United States and Canada, has not hitherto interfered with the subject, but has left it entirely in private hands. By means of this artificial method, streams, lakes, and ponds, in which the fish-supplies had either been reduced or removed, were re-stocked, and new forms introduced from distant waters.

While, therefore, the artificial hatching of freshwater fishes has thus been widely and successfully practised, it is otherwise with those inhabiting the sea. It is necessary, however, here briefly to review the situation in our country, in order to grasp the bearings of this important question.

Previous to 1883, our knowledge of the life-histories of the marine food-fishes was meagre. Few, or none, of our scientific men had studied their oviposition and development, so that such information had to be obtained by special inquiry for the Royal Commission then sitting (1883) under Lord Dalhousie. Fostered by the able and sympathetic chairman, and the other members of that commission, scientific knowledge of the fisheries generally, and of the development and life-histories of the marine food-fishes in particular, has made great progress; so that at the recent Committee on Fisheries of the House of Commons, under Lord Tweedmouth (an energetic member of the commission of 1883-84), the conditions were very different. This accurate knowledge of the subject has been obtained at the St. Andrews Marine Laboratory (under the Fishery Board for Scotland), the oldest in Britain; at the Granton Labora-

[1] Those familiar with this interesting spot will almost regret the transference of the main-hatching operations to Dupplin. The fry are now placed in the river at an early stage, and storage-ponds are unnecessary.

tory for a short time ; at the fine establishment of the Marine Biological Association at Plymouth, by the scientific members of the staff of the Fishery Board for Scotland, and by the recent expeditions of the Inspectors of Fisheries, Ireland, and the Royal Irish Academy. These united labours have not only made us acquainted with the spawning and development, as well as the whole life-histories of many of our most important marine food-fishes, have given us more extensive and accurate experience of the fisheries generally, but have stimulated practical energy in various directions, *e.g.*, in regard to shell-fishes and edible crustaceans. Much yet remains to be done, but those who had experience of the condition of our knowledge in 1883 will not be slow to accord due weight to the advances just alluded to, and especially to the labours of E. E. Prince,[1] T. Wemyss Fulton,[2] J. T. Cunningham,[3] and E. W. L. Holt.[4]

Such studies were absolutely necessary before proceeding, for instance, to hatch marine food-fishes on an extensive scale, and thus test the feasibility of increasing the numbers of the more valuable kinds by artificial methods. Moreover, they are equally important for purposes of legislation, in connection with the rate of growth and the attainment of maturity in fishes, the interdependence of the inshore and offshore waters—for example, the wafting of the eggs and larvæ of certain fishes shorewards, and the return of the young or adolescent fishes to the offshore waters ; while the food and the enemies of fishes at their various stages, the currents, temperatures, and the general physics of the water are also in need of further elucidation. These and many other problems are so closely interwoven with more purely scientific marine work that dissociation is impossible. Every nation with marine fisheries, indeed, is

[1] Now Commissioner of Fisheries in Canada.

[2] Superintendent of Scientific Investigations, Fishery Board for Scotland.

[3] Of the Marine Laboratory, Plymouth.

[4] Formerly of the St. Andrews Marine Laboratory, now at the Laboratory, Grimsby.

taking active steps to foster scientific inquiry in this direction, and contemplates, sooner or later, to carry out experiments in the hatching of marine food-fishes. Thus, for example, a year or two ago the German Government established at Heligoland an admirably equipped biological station for the scientific investigation of the North Sea fisheries.

It has yet, of course, to be proved that the artificial hatching of marine fishes, even on a large scale, will be beneficial to the fisheries generally; yet the importance of the issue demands an exhaustive trial. Almost everywhere during the last decade or two, complaints have been made as to the decrease of important marine food-fishes. Especially have the large halibut, turbot, brill, soles and other flat-fishes become rare. In Britain, this alleged diminution has been connected — for the last decade at least—with the extension of beam-trawling in our waters. Consequently, the legislature has closed the inshore area all round Scottish shores, and even considerably beyond that limit in certain places, as in the Moray Frith and the Frith of Clyde. Many thousand square miles of water are thus placed solely at the disposal of the liner.

The hatching of sea fishes on a large scale offers considerably greater difficulties than that of freshwater forms. The experimenter in his arrangements has occasionally to deal with the whole force of the waves in securing pure water for his tanks and ponds. Pumping is generally required, and the pipes and other apparatus must be specially constructed. Again, the necessary food for the minute and delicate young must be present in the tanks or other enclosures if he wishes to rear them beyond the larval condition; while predatory forms and decaying sea-weeds must be absent. Instead of young fishes, as in the salmon, which are capable of being handled with safety, the marine food-fish, with few exceptions, on its escape from the egg is a tiny, transparent creature, scarcely visible in the sea-water, and devoid of a mouth.

While here and there in our own and other countries

certain marine fishes had been hatched for scientific observations, yet to the Americans, under the late Professor Spencer Baird, belongs the credit of having first bestirred themselves in regard to the artificial hatching of marine food-fishes on a large scale. Since the establishment of the Fish Commission in 1871, various places on the coast have been selected for marine work of this kind, and finally the stations of Wood's Holl and Gloucester, on the shores of Massachusetts, were fixed on for permanent occupation and the erection of hatcheries, with, in the case of the former, a well-equipped laboratory. The Americans, indeed, with a breadth of view not always met with in our country, have ever held that it is not merely by the mechanical addition of fishes to our waters that the marine fisheries generally will be advanced. In other words, that it is on scientific investigation into marine life in general —whether directly or indirectly bearing on fishes and the fisheries, in addition to what may be termed fish-cultural work—that real progress depends. As Lord Playfair clearly put it in 1884: "Though the promise of practical utility from such (marine) laboratories is very great, that is not the first or the only thing to be considered. Laboratories of this nature, in which the habits of all kinds of marine life should be studied, ought primarily to be established, not with a view to practical uses, but with the main purpose of advancing science for its own sake. Science so studied rewards a nation a thousand-fold in the most unexpected practical applications; but without science there are no applications."[1]

As might have been expected, the hatching apparatus for the eggs of marine fishes was at first somewhat imperfect, and various methods—such as floating-boxes, plunging-buckets, Chester tidal-jars, and the universal hatching-jar—have since been adopted with more or less success. From the fact that most of the food-fishes have floating or pelagic eggs, which often rise near the surface of the water, quite a different arrangement is necessary from that, for instance,

[1] *Report of Meeting for Biological Investigation of Coasts of United Kingdom*, 31st March, 1884, p. 10.

required in the case of the herring with its adhesive eggs. The great principle is to keep the eggs in gentle motion amongst pure sea-water of the proper temperature. As first prominently brought forward by Lord Playfair, of St. Andrews, the Americans have chiefly experimented with the cod, and even so early as 1884 the fishermen on the neighbouring shores of the United States recognised the Commissioner's labours in shoals of young fishes which they termed "Commission cod". To a small extent, however, they have also dealt with the eggs of pollack, haddock, sea-bass (*Serranus*), Spanish mackerel, common mackerel, scup (*Stenostomus*), squeteague (*Cynoscion*), tautog (*Tautoga*), cunner (*Ctenolabrus*), with the sand-dab and the spotted flounder, both of which, like all the preceding, have buoyant or pelagic eggs, and with a flounder (*Pseudopleuronectes Americanus*), which has demersal (*i.e.*, non-floating) adhesive eggs. The anadromous shad has also been hatched in enormous numbers, and with very great success, but this form does not so much concern us at present. Altogether (up to 1891), larval cod to the number of eighty millions have been placed in the sea by the Commission; and Colonel Marshall Macdonald, the distinguished head of the department, is of opinion that as the result of these operations cod have appeared in places where they were formerly unknown. He specially points out that a variety with dark spots on the back, and hatched at Gloucester marine station, had been got for the first time in the waters near Plymouth Harbour in schools of fishes of about four pounds weight.

In the same continent, another station for the hatching of the ova of the cod has been for more than three years in operation at Dildo Island, Newfoundland, under the government of Canada. Up to 1893 (and for the three years) two hundred and twenty-two millions of larval cod had been placed in the water in an increasing ratio year by year. Thus in 1893, two hundred and sixty-five millions of the ova of the cod were placed in the hatching-vessels, and one hundred and sixty-five millions of larval fishes deposited in the sea, or about 62¼ per cent. At this station

the success in hatching the eggs of the lobster, a form rapidly diminishing in numbers both in Canada and the United Kingdom, has been noteworthy. Thus in the floating-boxes or incubators in 1893, four hundred and eighty-four millions two hundred and eighty-six thousand eggs of the lobster gave four hundred and twenty-seven millions two hundred and eighty-five thousand young lobsters, or 88·2 per cent., while in the hatchery proper another series of two millions five hundred thousand larval lobsters were produced. These valuable crustaceans are therefore readily hatched, but the difficulties begin shortly afterwards, since they devour each other so ravenously that the mortality is serious. Professor Prince, the Canadian Commissioner of Fisheries, indeed informs me that it is found necessary, at an early stage, to convey them in casks of sea-water to suitable sites in the inshore grounds, and cautiously sink them. As the casks have a large hole on one side, the young lobsters can freely escape at leisure into the ocean to follow their temporary pelagic existence. Professor Prince is of opinion that the Wilmot jar is, perhaps, the most successful apparatus for the hatching of these eggs. In Britain, it would be a very difficult task, even at Barra, to procure a tenth of the foregoing number of eggs, which are supplied by the various lobster "canneries" in the neighbourhood. In regard to the effects of these experiments on the surrounding waters, the fishermen attribute the shoals of young cod to the operations of the hatchery; but no observation of note appears yet to have been made concerning the lobsters.

Coming now to the continent of Europe, it is found that Norway, long so famous for its great cod-fisheries, holds a prominent place in marine fish-hatching, mainly through the efforts of Captain Dannevig, who, since 1884, has had charge of the sea-fish hatchery at Flödevig, Arendal. Year by year this energetic official has improved his methods, so that since the hatchery was established no less than nine hundred millions of larval cod have been placed in the fjords of the neighbourhood. Dannevig has also successfully reared the young cod in his enclosed ponds

to the second year. Constant fishing had greatly thinned the small variety of cod frequenting the Norwegian fjords, and there should be less difficulty, therefore, in determining the result of these experiments. For a year or two it is stated that the increase of cod on the inshore grounds has been marked, and further reports on the condition of these waters will be anxiously watched. Besides hatching the cod on a large scale, Dannevig has experimented successfully with the eggs of the flounder and the lobster. The apparatus used by Dannevig is that mainly employed at Dunbar, and will subsequently be described. In addition to the extensive hatching-house, a large salt-water basin for rearing the young, and a pond for the spawning fishes, are attached to the Norwegian station.

Though this country is thus not the first to make experiments in hatching marine fishes on a large scale, it is ten years at least since both this and other means of increasing the more valuable food-fishes were brought forward. Last year one of the methods was carried out by bringing several hundred soles from the east coast of England and placing them in St. Andrews Bay, where only a very few occur. This year the Fishery Board for Scotland was also able to commence the artificial hatching of sea fishes at Dunbar, where for some time they have been making preparations by constructing a hatching-house, spawning-pond, filtering-boxes, and by enclosing a tidal-creek for retaining the spawning fishes. As a careful account of the arrangements, which were founded on those of Captain Dannevig at Arendal, is given in the Report of the Fishery Board by Dr. Fulton,[1] with whom rested the responsibility of carrying out the whole system, only such structural features as are necessary to explain the method will be alluded to.

In selecting the species to be experimented with at Dunbar, it was thought desirable to begin with forms not only valuable in the market, but comparatively scarce in the neighbouring waters, and accordingly the turbot and sole were chosen. The former is, perhaps, the most valuable

[1] *Twelfth Annual Report* (1894), p. 196. Also Captain Dannevig's *Beskrivelse af Flodevigens Udklœkningsanstalt ved Arendal*, 1892.

flat-fish in the British seas, and, moreover, it is tolerably hardy, so that it can be reared from young stages to the adult condition in tanks and confined areas of sea-water. The latter is also a valuable food-fish, and is hardy at all stages, while its comparative rarity in the neighbouring waters will make any increase after such operations unmistakable. A considerable number of soles had been collected last autumn and kept in the enclosed tidal creek at Dunbar, but as their spawning-season is somewhat late, it was decided to occupy the early spring months by experimenting with the plaice.

Steps were taken to secure adult plaice from the offshore grounds early in the season by aid of the *Garland*, and others were obtained on board various trawlers, though many of these afterwards succumbed from injuries received in the trawl. When the period for transference to the spawning-pond arrived, it was found that of fifty-eight adult plaice which had been confined for some months in the tidal-creek and had thus become acclimatised to their new life, all survived, while there was considerable mortality amongst those recently received. The greatest number of plaice in the spawning-pond at a given time was three hundred and ninety, and the whole number experimented with three hundred and ninety-six—a comparatively smal total when contrasted with the large number of cod kept in confinement during the operations of the Americans. The average size of the males was $17\frac{3}{8}$ inches, and of the females, $20\frac{7}{8}$ inches. The fishes fed readily in the spawning-pond, and swam actively about.

Spawning commenced in earnest on the 9th March, on which day two hundred and twenty-five thousand ova were collected, and continued till the 8th May, the total number of ova obtained during this period being twenty-seven million three hundred and fifty thousand. These large eggs had been hatched at the St. Andrews laboratory every season since 1884, and the larval fishes were known to be hardy, so that a good return was expected. Accordingly it was found that the number of larval fishes placed in the sea was twenty-six million and sixty thousand, the

loss during the operations in hatching being thus extremely small, *viz.*, only 4·4 per cent., a success largely due to the unceasing care of Dr. Fulton, Superintendent of scientific investigations of the Board, and to the enthusiasm and practical skill of Mr. Harald Dannevig, who had charge of the establishment. This result contrasts favourably with any hitherto obtained, especially when it is remembered that everything was new at Dunbar. If we, for instance, examine the American experiments at Wood's Holl during the season 1889-90, it is found that three thousand four hundred and three adult cod had been collected and retained in tidal-basins and live-cars for spawning purposes. Many of these, however, died or became blind, leaving at the end of the spawning-season only one thousand adults. From this large number of fishes, only eight million five hundred and forty-five thousand seven hundred ova were obtained, the mortality, further, during the hatching process being about 30 per cent., so that five million eight hundred and sixty-one thousand one hundred larvæ (fry) were liberated in the adjoining sea. Even a larger loss occurred in the case of one million one hundred and thirty-eight thousand two hundred ova of the haddock, for only five hundred and twenty-eight thousand larvæ were produced. A better result, however, took place with five million eight hundred and forty-one thousand one hundred eggs of a flat-fish (probably *Pseudopleuronectes*), since four million and eighty-six thousand seven hundred larval fishes were obtained for liberation. At Dildo hatchery in Newfoundland, the eggs of the cod have also been dealt with, and apparently with greater success than at Wood's Holl, for in 1892, the third year of the experiments, the total number placed in the sea when about a week old was one hundred and sixty-five millions, or nine times more than the first season. The loss, even in the latter instance, is greater than in the case of the plaice at Dunbar.

Again, at Wood's Holl, three thousand adult cod yielded from November, 1890, to February, 1891, sixty-seven million three hundred and ninety-nine thousand ova, from which thirty-six millions of larval fishes were obtained,

the mortality being thus very serious. In the instance of forty-three millions of ova sent by rail from Gloucester station, only one hundred and fifty thousand larvæ were forthcoming for liberation. The same species of flounder as in the preceding season gave four million six hundred and eighty-nine thousand seven hundred ova between February and March, from which issued three million three hundred and fifty thousand eight hundred larval fishes.

At Gloucester marine station in 1889-90, forty millions of the ova of the pollack produced fourteen millions of larvæ. From forty-seven millions of the eggs of the cod fourteen millions of larvæ were obtained ; while from thirty millions of the eggs of the haddock, only five millions of larvæ were liberated. No statistics are given for 1890-91, except that thirty millions of the eggs of the cod collected in the neighbourhood, and, therefore, favourably situated, gave 25·5 per cent. of larvæ.

At Dunbar, the ova are voluntarily shed in the spawning-pond, which is forty and a half feet long by eleven feet two inches deep, the breadth varying from twenty-six and a half feet at the one end to eighteen feet at the other, and into which clean sea-water is sent in a full stream from a three-inch pipe of galvanised iron. The current carries them to a trough called the spawn-collector, which at first had only a frame of horse-hair cloth, but the eggs are now more expeditiously collected by a fine gauze bag through which the water passes, and which only requires to be gently inverted into a suitable vessel of sea-water to secure the eggs in perfect condition. Before collecting the eggs for the day, a larger current is made to pass out of the pond, thus increasing the number of eggs in the collector. The ova are measured and counted, and then placed in the hatching-boxes, each of which has the same length and depth, *viz.*, eleven and a half inches, and ten and three-quarter inches broad, while the bottom is of hair-cloth.

The boxes are attached by leather hinges to the partitions, and when the troughs are full of water the free end rises until it projects about three inches above the water,

and here comes in one of the features of Dannevig's system. The water which carried the ova to the spawn-collector rushes down an incline to a water-wheel, which moves an eccentric wheel or cam in the hatching-house. This gives an up-and-down movement to a bar of iron which passes between each series of hatching-boxes (five in number on each side), and has five short transverse pieces, each of which rests on a box. When the rod is raised by the wheel, the buoyancy of the boxes causes them to rise as already mentioned; but twice every minute the rods fall by the action of a beam on the cam, and each hatching-box is pressed under water by the transverse piece resting on it. Thus, besides the lively current of sea-water constantly flowing in by the broad metal spout at the upper end of the box, the rush of water through the horse-hair cloth on the bottom causes the ova to be in constant movement, and very regularly distributed throughout the box. The sea-water for this purpose is passed through a series of flannel filters arranged on each side (*i.e.*, in duplicate) of the slope between the spawning-pond and the hatching-house, and thus perfect purity is secured. Nowhere could healthier eggs be seen, and when the characteristic yellow pigment appeared in the embryos within them they were even beautiful. At the beginning of the work and when the temperature of the water had an average of 5·24° C., Mr. Dannevig found they hatched in twenty-one days, while towards the end of the operations and when the average temperature was 8·86° C., they issued in fourteen days. He kept some in the vessels till the yolk was absorbed and the mouth open, and for a considerable time thereafter, in all for forty-seven days, when an unfortunate accident to the water swept them off. Shortly after hatching (a day or two), the larval plaice turn their heads against the stream and keep in this position, as so frequently seen in the case of the young salmon at Stormontfield. The larval fishes, as a rule, are kept in the same compartments until the yolk is nearly absorbed, that is, from ten to fourteen days. They are then conveyed by the steamer *Garland* to the areas selected in boxes, which are

gently lowered into the water, and the little fishes soon spread themselves around.

On the other hand, the Americans kept the fry of the cod only from twelve to forty-eight hours after hatching. Here it was thought prudent to liberate the larval plaice at distances varying from two to six miles from shore, so that the usual transformation might ensue before the latter was reached. Under present circumstances, no question could be raised as to the retention or dismissal of the larval fishes, since no pond was available. Moreover, it is an open question whether the retention of the young fishes—until they have attained some size—would be a marked advantage. The expense would certainly be greater. Nowhere can these minute fishes find such nourishment as unfailingly meets them in the sea, and nowhere will they grow more rapidly. It is true that, translucent though they be, their natural enemies will reduce their numbers, but such is unavoidable. For the elucidation of the life-histories, again, of some, *e.g.*, the haddock and lobster, and the rate of growth of others, a certain number can be experimented with in tidal-ponds, as soon as these are available.

There is, perhaps, no urgent need for taking the round fishes in hand at present, since cod and haddock are in fair abundance, but if it were possible to increase the numbers of halibut, turbot, and soles, the benefit to the fishing community and the public would be great. The difficulties connected with the artificial hatching of these on a large scale are somewhat greater than in the case of the round fishes, but it is to be hoped they will be overcome. If it be found that the sole will live and breed in fresh water, and that the turbot, the brill, the bass, and the grey mullet—as stated so far back as 1837—will live in a five-acre lake so fresh that cattle drank it for nine months of the year, encouragement of a welcome kind is given to these hatching operations. Again, by the acclimatising of certain fishes, *e.g.*, the Baltic herring, to fresh water, a more successful means of transmitting the species to distant seas may also be offered than by the attempt to forward the eggs.

Amongst the pelagic eggs, besides those above men-

tioned, specially suitable for hatching on a large scale, are those of the hake, ling, torsk, lemon-dab, cod, haddock, whiting, bib, green cod, pollack, brill, dab, long rough-dab, sail-fluke, and gurnard. The large eggs of the wolf-fish (cat-fish of the fisherman), which adhere together in masses, can also be hatched with ease, and the young are remarkably hardy. Again, the eggs removed from "berried" lobsters may be hatched in the foregoing boxes or in Wilmot jars, and the larvæ either kept in extensive ponds (where, however, their habits of cannibalism are troublesome), or, as in Canada, carried seawards and sunk in large barrels with a hole on one side; and since great numbers are required, the neighbourhood of good lobster-ground, as at Dunbar or Barra, would be an advantage.

The establishment at Dunbar just referred to is thus a small beginning in this important field. We have only stepped on the threshold of the subject. A liberal and wise expenditure is absolutely necessary to carry out the experiments on a scale sufficient to test their real influence on the fisheries. Moreover, not only are skill, enterprise, and perseverance indispensable on the part of the officials, but patience on the part of the public. Those most familiar with the department, while, perhaps, by no means sanguine, feel that we have now reached a stage when the subject calls to be dealt with in a comprehensive manner. It is the practice in certain quarters to sneer at the supposed small results science can show, especially in Scotland, for the expenditure in connection with the fisheries. The total sum, however, for scientific investigations is only £3000; and if the cost of the small steamer *Garland* required for the survey and protection of the great areas (thousands of square miles) now reserved only for the liners, be deducted, the comparatively small sum of £1800 a year remains. This income has to bear the salaries of the staff, experiments in fish-, lobster-, and mussel-culture, the cost of apparatus, the marine laboratory at St. Andrews, the hatchery for marine fishes with its small laboratory at Dunbar, and the carrying out of other scientific fishery work. In England, for instance, the official expenditure in

the Fisheries Department of the Board of Trade nearly reaches the first sum (£3000), and yet no scientific investigations, no expenditure for a steamer, and no marine fish-hatching have to be met. Indeed, another £1000 is given by the Government to the marine laboratory at Plymouth for scientific fisheries' work, making the annual income of this establishment about £2200. As Dr. Fulton has shown, the United States spends annually £70,000 on fish-culture and scientific investigations, and employs two large steamers and a sailing vessel exclusively for the work. Besides this large sum, the fish commissioners of the various States also disburse considerably on the development of their fisheries. Canada, again, expends £100,000 yearly on her fisheries, of which a sum of about £10,000 is devoted to fish-culture.[1]

Finally, were the establishment of a proper series of statistics the only benefit that had already accrued to the fisheries of the country since scientific attention had been directed to the subject, such would be no trifling boon. Overweening confidence on the one hand, and pessimistic views on the other, have been toned to moderation, and at least a basis for guidance afforded where only conjecture existed. But during the last decade the studies on the reproduction, development, and life-histories of our food-fishes and shell-fishes, and the distribution of the immature fishes, have made it possible to proceed with the artificial culture of them. Much knowledge has also been gained concerning the rate of growth, the size at which maturity occurs, the food—both on the bottom of the sea and in mid-water—the latter in abundance at all periods of the year, and often presenting no connection with locality. Besides, in questions more or less practical, such as the

[1] Keeping the £1800 for Scotch scientific fisheries' investigations in view, a glance at the estimate for sundry grants in aid of scientific investigations, etc., in the United Kingdom (year ending 31st March, 1895), and amounting to £26,247, may be taken. Of this sum, £21,800 goes to England, £2877 to Ireland, and £1570 to Scotland. But, in addition, provision is made in other estimates for expenditure in connection with such service of £17,834, the total thus being £44,081.

formation of a mackerel fishery, the capture of immature fishes, and the influence of beam-trawling on the fishing-grounds and the abundance of food-fishes, important results have been obtained. These and other investigations, moreover, are watched with the closest attention by the fishery authorities on the continent and America.

It is true much yet remains to be done ; thus, even the ripe eggs and larval stages of such ubiquitous forms as the eel and the conger are unknown. It is certain, however, that a store of knowledge is being gradually accumulated—knowledge not only of value now, but that will be permanently advantageous to the department.

W. C. McIntosh.

THE MOLECULAR WEIGHT OF LIQUIDS.

THE properties of matter may be conveniently divided into three main classes: i., Additive; ii., Constitutive; iii., Colligative.

In determining the molecular weight of a substance, no matter whether it be gaseous or liquid, it is necessary to make use of some colligative property of the matter under investigation. The present article will therefore deal almost exclusively with colligative properties, or rather with a particular colligative property of liquids. As regards the molecular weight or complexity of solid substances it is well to state, at the outset, that our stoichiometrical knowledge of the properties of solids is so limited that up to the present time we have absolutely no reliable information on this point.

If a substance plays a part in a chemical phenomenon in such a way that any particular property of the substance remains unaltered or independent of its state of chemical combination, then this property is said to be additive. The best known additive property of matter is its *mass*. Thus, the mass of a substance undergoes no alteration during a change in its state of chemical combination. This fact finds its expression in the well-known law of the "Conservation of Matter". Properties, such as melting- and boiling-points, molecular volume, etc., which depend to a large extent on the arrangement of the atoms in the molecule, have been termed constitutive. It very often happens, however, as in the examples just quoted, that a substance exhibits properties which are partially additive and partially constitutive.

The third group of properties is characterised by the fact that for chemically comparable quantities of the most widely different substances, a colligative property has always the same value. Whilst the mass of a compound is the sum of the masses of the elements of which it is composed, the volume of a gas, under the same conditions of temperature and pressure, is a colligative property and is

quite independent both of the chemical nature of the gas and of the number of atoms in its molecule. If we take a certain volume of hydrogen and convert it into water gas, H_2O, the volume remains unaltered. Further if we suppose that by adding ethylene C_2H_4 we could convert the water into alcohol gas C_2H_6O, the volume would still remain unaltered. By adding ethylene once more we may suppose that butyl alcohol, $C_4H_{10}O$, is formed. The volume of butyl alcohol gas would be precisely that of the hydrogen from which we started, and so on.

Just as the presence of additive properties in matter forms the foundation of the Atomic Theory, so the appearance of colligative properties forms the basis of the Molecular Theory. Colligative properties may be explained on the assumption that there exist certain independent groups of atoms, the molecules, which behave entirely in virtue of their number and irrespective of their nature and chemical composition. These colligative properties therefore lead to the molecular theory, and every such property may be made use of in order to determine the relative complexity or number of the molecules.

For example, according to Avogadro's law, equal volumes of gases, under the same conditions of temperature and pressure, contain the same number of molecules. This law may also be stated in another way, namely :—

Under the same conditions of temperature and pressure the molecular weights of gases are directly proportional to their densities.

It is thus easy to determine the molecular weight of gases from their densities, the molecular weight of hydrogen being chosen = 2 for reasons which are well known to chemists.

If it were found necessary to determine the molecular weight of a gas without a knowledge of its density, this could be done by measuring the rate of alteration of its volume energy with temperature. The volume energy of a gas is the product of its volume and pressure, pv; and if we choose as the unit of volume of a gas, the volume in litres occupied by the molecular weight expressed in grams, and

as unit of pressure, an atmosphere, then the volume energy of a gas is equal to a constant multiplied by the absolute temperature, thus : $pv = RT$. The constant R, for all gases which do not dissociate, is 0·0819. If, then, in investigating an unknown gas, we happened to find that its volume energy increased proportionately to the absolute temperature, and that the proportionality factor was 0·0819, we should be justified in concluding that the molecular weight chosen in expressing v was the correct one. But, if the proportionality factor were $2 \times 0{\cdot}0819 = 0{\cdot}1638$ we should then conclude that the number chosen for the molecular weight, and consequently the volume of the gas, was twice as great as it should have been.

If the factor had a value intermediate between 0·0819 and 0·1638 the deduction would follow that we were dealing with a mixture, and, finally, if the value of the factor were found to vary with rise of temperature, we should conclude that the composition of the mixture was altering. The volume energy would then be no longer a rectilinear, but a curvilinear function of the temperature, and the composition of the mixture could be deduced at any given temperature by a comparison of the slope of the tangent to the curve at that temperature with the slope of the normal line, expressed by the number 0·0819.

This method offers no advantage over the ordinary method for determining the molecular weights of gases, but an account of it has been given here, as an analogous method has been employed to determine the molecular weight of liquid substances.

To the Hungarian physicist Eötvös belongs the credit of having first shown (1886) that what has since been termed the *molecular surface energy* by Ostwald, is, between certain limits, a linear function of the temperature. Eötvös also observed that there were certain exceptions to the rule, and he pointed out that these exceptions could be made to disappear by choosing appropriate molecular weights which were higher than those ordinarily accepted.

Reference has already been made to the gas equation $pv = RT$, where p is the pressure of the gas, v the volume contain-

ing equal numbers of molecules, R a constant, and T temperature reckoned upwards from absolute zero, where pressure disappears. Ramsay and Shields have pointed out that there exists an analogous equation for liquids. If we represent the surface tension of a liquid by γ, the surface on which equal numbers of molecules lie by s, a constant analogous to R by k, and temperature measured downwards from the critical temperature, where the surface tension becomes zero, by τ, then the analogous equation for liquids is, $\gamma s = k\tau$. To this equation, however, a slight correction in the shape of another constant, d, must be added, as the line representing the product of surface tension into molecular surface has its origin, not at the critical temperature, but usually about $d = 6^\circ$ below it. The equation, therefore, becomes $\gamma s = k\,(\tau - d)$.

As gases have never been investigated at temperatures closely approaching the absolute zero we have no means of knowing whether a correction, similar to d, should be applied or not.

All the terms in the above equation are either known or susceptible to measurement. Eötvös determined the surface tension by an optical method, Ramsay and Shields by measuring the capillary rise in a tube of narrow bore.

In order to obtain good results several refinements of the ordinary methods are necessary, for the details of which the original memoirs should be consulted. In a good many instances the surface tension has been determined for a range between the ordinary temperature and the critical temperature; in some cases even as low as -90°C.

Assuming that the distribution of the molecules is the same on the surface of a liquid as in the interior, the next term in the equation, s, the surface on which equal numbers of molecules lie, is determined as follows. If v is the specific volume of the liquid and M its molecular weight, then Mv is its molecular volume, that is to say, the volume in cubic centimetres occupied by a gram-molecule of the liquid. Now, let us conceive a cube with a volume equal to the molecular volume of the liquid; the cube root of the molecular volume, on the above assumption, obviously gives

the relative number of molecules along a side of the cube, whilst the square of this number represents the relative number of molecules distributed on a face of the cube. For different liquids, therefore, the molecular volume raised to the power two-thirds $(Mv)^{\frac{2}{3}}$, represents the surface on which the *same* number of molecules lies. This surface s is sometimes called the molecular surface of the liquid, so that instead of s in the above equation we may substitute $(Mv)^{\frac{2}{3}}$. The critical temperature and the correction d being determined, we have all the requisite data for calculating the value of k by the equation $\gamma\,(Mv)^{\frac{2}{3}} = k\,(\tau - d)$.

After carefully investigating a series of liquids Ramsay and Shields found that the mean value of the constant k was 2·121. All liquids, however, as Eötvös also found, do not give constants approaching the value 2·121. The chief exceptions are water and the fatty acids and alcohols, which have values less than 2·121. It is noteworthy that in the case of the apparent exceptions, the values of k vary with the temperature and more nearly approach the normal value the higher the temperature. In other words the molecular surface energy, the product of the surface tension γ into the molecular surface $(Mv)^{\frac{2}{3}}$, is no longer a linear function of the temperature.

Returning once more to the equation $\gamma\,(Mv)^{\frac{2}{3}} = k\,(\tau - d)$, it follows that the quotient of the differences of the molecular surface energy between any two temperatures by the difference in temperature, should remain constant, that is—

$$\frac{\gamma(Mv)^{\frac{2}{3}} - \gamma'\,(Mv')^{\frac{2}{3}}}{\tau - \tau'} = k,$$

γ and v corresponding to the temperature τ, and γ' and v' to the temperature τ'.

In the case of a great many liquids this was found to be the case. The variation in the constant from 2·121 lies within the limits of experimental error in the case of such compounds as the esters, the hydrocarbons, carbon tetrachloride, chlorobenzene, ether, mercaptan, the mustard oils, chloral, paraldehyde, benzoic aldehyde, nitrobenzene, aniline, pyridine, quinoline, silicon tetrachloride, phosphorus trichloride, phosphorus oxychloride, sulphur

chloride, thionyl chloride, sulphuryl chloride, nickel tetracarbonyl, etc.

It has already been pointed out that, in the gas equation $pv = RT$, the value of R is 0·0819, and that it does not vary with the temperature provided the molecular weight of the gas has been correctly chosen and that no dissociation occurs. If, however, dissociation takes place the value of R increases with rise of temperature. As we shall see later on, there is a great deal of evidence to show that the molecules of certain liquids, in passing from the state of vapour to that of liquid, associate to form more complex molecules, and that these liquids are precisely those which possess an abnormal and variable value of k. The effect of association would be that the real molecular weight would be chosen too low, and consequently that the value of k would appear to be less than 2·121. Furthermore, if such an associated liquid dissociated on heating, the low abnormal value of k would gradually increase. This is, in fact, what actually takes place, and Eötvös pointed out that the degree of association or the molecular complexity of the liquid could be calculated on this basis. The following experimental results, for example, have been obtained by Ramsay and Shields :—

Methyl Alcohol.		Ethyl Alcohol.		Formic Acid.		Acetic Acid.	
Temp. Interval.	k.	Temp. Interval.	k.	Temp. Interval.	k.	Temp. Interval.	k.
16°- 46°	0·933	16°- 46°	1·083	17°-46°	0·902	16°- 46°	0·900
46°- 78°	0·969	46°- 78°	1·172	46°-80°	0·911	46°- 78°	0·953
78°-132°	1·046	78°-132°	1·352	—	—	78°-132°	1·074

It will be seen that the value of k differs widely from 2·121 and that it gradually increases with rise of temperature.

The question which now rises is how the degree of

association or molecular complexity at any particular temperature can be calculated. We have already seen that the behaviour of non-associating or "normal" liquids may be expressed by the equation—

$$\frac{\gamma'(Mv)^{\frac{2}{3}} - \gamma'(Mv')^{\frac{2}{3}}}{\tau - \tau'} = 2{\cdot}121,$$

whilst for associated liquids—

$$\frac{\gamma(Mv)^{\frac{2}{3}} - \gamma'(Mv')^{\frac{2}{3}}}{\tau - \tau'} = k,$$

where k is a number varying with the temperature, and less than 2·121.

At each particular temperature we can multiply the gaseous or normal molecular weight of the liquid by a factor x, which is chosen so that the k of the last equation becomes 2·121 ; that is to say, we can select a molecular weight for the liquid which gives the normal constant 2·121.

The factor x at each temperature represents the molecular complexity of the liquid at that temperature. At present, unfortunately, there is no way of verifying this method of calculating the molecular complexity of liquids. It is still open to adverse criticism on the score that no rigorous proof has been brought forward to show that the true molecular complexity is obtained in this way, but it must be admitted that we can get an approximation to the molecular complexity, and future investigations will show how close the approximation is.

In the case of the normal liquids the experimentally found values of the molecular surface energy can be reproduced by the equation $\gamma(Mv)^{\frac{2}{3}} = 2{\cdot}121\ (\tau - d)$ up to within about 30° from the critical point, and by a slight alteration it may be made to fit right up to the critical point.

By the addition of another constant μ it was found that a formula of the form—

$$\gamma(Mv)^{\frac{2}{3}} = \frac{K(\tau - d)}{1 + \mu\tau}$$

agrees admirably with the experimental values of the molecular surface energy of methyl and ethyl alcohols, water

and acetic acid, between low temperatures and about 30° from their critical points. In the case of methyl and ethyl alcohols the agreement extends even as low as − 90°C. Agreement near the critical temperature may also be secured by a slight alteration in the formula. Neglecting, for the present, the slight alteration, the constants for these associating substances are :—

	K	d	μ	Critical Temperature
Methyl alcohol	1·489	−4·22	0·00104	240·0
Ethyl alcohol	2·170	4·8	0·00193	243·1
Water	2·631	19·5	0·00218	358·1
Acetic acid	1·910	11·9	0·00163	321·5

Having found a formula which reproduces the experimentally found values of the molecular surface energy of associating liquids at different temperatures, we are in a position to calculate the degree of molecular complexity of the liquid in the following manner. Supposing that the liquid is composed partly of complex molecules and that x is the measure of complexity, we have, as before—

$$\gamma\,(x\mathrm{M}v)^{\frac{1}{3}} = 2{\cdot}121\,(\tau - d),$$

or—

$$\gamma\,(\mathrm{M}v)^{\frac{1}{3}} = \frac{1}{x^{\frac{1}{3}}} \times 2{\cdot}121 \times (\tau - d).$$

But, according to the formula we have just found—

$$\gamma\,(\mathrm{M}v)^{\frac{1}{3}} = \frac{\mathrm{K}\,(\tau - d)}{1 + \mu\tau};$$

so that by equating the two right-hand sides of the equations and simplifying we get—

$$x = \left\{ \frac{2{\cdot}121}{\mathrm{K}}\,(1 + \mu\tau) \right\}^{\frac{3}{2}}.$$

The degree of association x, of methyl and ethyl alcohols, water and acetic acid, at different temperatures has been calculated in this way and the results are given in the following table :—

Methyl Alcohol.				Ethyl Alcohol.				Water.				Acetic Acid.			
$t.$	$\gamma(Mv)^{\frac{1}{3}}$.		$x.$	$t.$	$\gamma(Mv)^{\frac{1}{4}}$.		$x.$	$t.$	$\gamma(Mv)^{\frac{1}{3}}$.		$x.$	$t.$	$\gamma(Mv)^{\frac{1}{4}}$.		$x.$
	Found.	Calcd.			Found.	Calcd.			Found.	Calcd.			Found.	Calcd.	
− 89·8	361·8	369·8	2·65	− 89·8	436·1	433·3	2·03	0	502·9	503·5	1·71	20	371·2	371·1	2·13
+ 20	271·4	271·4	2·32	+ 20	331·0	331·0	1·65	20	485·3	484·9	1·64	80	—	314·8	1·92
90	196·3	198·6	2·11	100	235·0	235·1	1·39	60	446·2	446·4	1·52	140	250·2	250·0	1·72
150	131·3	128·3	1·94	160	147·2	146·4	1·21	100	403·5	403·5	1·41	200	174·9	174·7	1·53
200	60·6	63·2	1·81	220	39·2	38·0	1·03	140	357·0	355·6	1·29	280	54·8	52·9	1·30

In the table the temperatures are given under t, the observed $\gamma\,(Mv)^{\frac{1}{3}}$ under the column headed "found," the calculated $\gamma\,(Mv)^{\frac{1}{3}}$ using the constants given above, under "calculated," and the molecular complexity under x. Methyl alcohol is the only substance at ordinary temperature having a complexity much greater than 2. The molecular complexity of the other substances is usually rather less than 2, and becomes less and less as the critical point is approached.

Without going to the trouble of finding a formula which represents the variation of molecular surface energy of associating liquids with temperature, it is possible to obtain a fairly good approximation to the degree of molecular complexity from the equation—

$$\gamma\,(Mv)^{\frac{1}{3}} = \frac{1}{x^{\frac{2}{3}}} \times 2{\cdot}121 \times (\tau - d)\,;$$

but this involves the assumption that the actually found critical temperature of the liquid is not far removed from the critical temperature of the liquid, supposed to consist of simple molecules. Thus we obtain—

$$x = \left\{ \frac{2{\cdot}121\,(\tau - d)}{\gamma\,(Mv)^{\frac{1}{3}}} \right\}^{\frac{3}{2}}.$$

The results obtained in this way differ very little from those calculated by the formula—

$$x = \left\{ \frac{2{\cdot}121}{K}\,(1 + \mu\tau) \right\}^{\frac{3}{2}};$$

so that when only a few data are at hand this formula will generally be found to give a very fair idea of the molecular complexity of the liquid.

The chief result which has been gained by the investigations on the surface tensions of liquids is that liquids have been divided into two groups. One group contains the normal or unassociated liquids, the other those which form complex molecules or are associated. The degree of complexity of the associated liquids as given in the previous table is probably correct within certain limits, which are conditioned by the fact that the number chosen for the constant 2·121, is not absolutely constant but varies with the nature of the compound.

The extreme variation for the fourteen substances which have been most carefully investigated is between 2·020 for ethyl formate and 2·248 for methyl isobutyrate. On the assumption that this is the extreme divergence, there may be a positive or negative error of 5 per cent. caused by assuming the mean value to be 2·121.

In conclusion a short review of the evidence which proves in a general way that certain liquids, and especially the fatty acids, alcohols and water, possess complex molecules may not be out of place.

Guye has shown that the critical co-efficient of a liquid, that is, the quotient obtained on dividing the absolute critical temperature by the critical pressure, should, when multiplied by the constant 1·8, be equal to the molecular refraction.

Whilst the majority of substances examined by Guye appear to consist of simple molecular groups at their critical points, water, methyl alcohol and acetic acid yield numbers which point to association, inasmuch as the constant, instead of having its usual value 1·8, has decreased to about 1·1.

Young and Thomas have shown that the densities of most liquids at their critical points may be found by multiplying their theoretical densities by a number approximately equal to 3·85. Methyl, ethyl and propyl alcohols and acetic acid are, however, exceptions to this rule, inasmuch as the factor varies between four and five. This fact again points to the presence of complex molecules at the critical temperature.

In determining the true volume of water and of the above liquids at their critical points by the method suggested by Cailletet and Matthias, the same authors again find evidence of association in the liquid state.

When the numbers representing heats of vaporisation of a compound increase to a maximum and then diminish, Guye has shown from Ramsay and Young's work that the compound contains complex molecules in the liquid state. This was found to be the case with ethyl alcohol and acetic acid. The fact that the vapour pressure curve of a liquid cuts those of undeniably simple substances, such as benzene,

carbon tetrachloride, etc., may be taken as proof that the liquid contains complex molecules. Evidence of a like nature has also been brought forward by Thorpe and Rodger during their investigation of the viscosity of liquids.

This must suffice to show that between normal liquids, such as the esters, on the one hand, and the fatty alcohols and acids on the other, there exists an important difference. The characteristic difference between normal and associated liquids is, that in the case of the former the molecular surface energy is a linear function of the temperature, whilst in the latter case it is not so.

It has been assumed that the molecular weight of the normal liquids in the liquid state is the same as that of their gases. There is no positive evidence to show that the molecules of such liquids as follow the law do not associate into twos or threes on assuming the liquid state. One thing however is certain: if they do, they all associate to an equal extent, and the degree of association is not altered by rise of temperature. It is unlikely that mere liquefaction should produce in almost all cases equal association, and it is unlikely that a rise of temperature should not cause the dissociation of an associated body.

It is evident, however, that the assumption that the molecular weights of such liquids as the esters are not changed on assuming the liquid state is a mere hypothesis; but it is one for which there is a great deal of probability, and probability of the same kind as that which led to the adoption of the usually received molecular formulæ for gases.

It is now pretty generally known that the molecular weight of liquids is not the same thing as the molecular weight of liquids in liquid solution. Van't Hoff has shown that dissolved substances behave as gases, in obeying the well-known gas laws, if we only substitute osmotic for ordinary gas pressure. The molecular weight of a liquid as determined by the cryoscopic or ebullioscopic method, for example, is therefore not to be considered the same as the molecular weight of the liquid *per se*, but rather the same as the molecular weight of the substance in the gaseous

state, provided, of course, that the solvent itself has not the effect of producing molecular aggregation of the dissolved substance.

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THE ORIGIN OF THE VASCULAR PLANTS.

ABOUT forty years ago there appeared a series of publications that marked an epoch in the study of vegetable morphology. These were the remarkable papers of the German botanist Hofmeister, in which the comparative morphology of what are now known as the Archegoniatæ was discussed for the first time from the standpoint of the evolutionist. Hofmeister showed that the Mosses and Ferns belonged to one natural series, and that the Phanerogams were connected by the Gymnosperms directly to the higher Pteridophytes, and that the terms "Cryptogam" and "Phanerogam" must be discarded, at least in their old signification, as marking two radically different types of plants.

The result of Hofmeister's work was the arousing of extraordinary interest in the study of the structure and development of the Archegoniatæ and the publication of a great number of investigations upon these forms. It is not necessary to mention here the long list of eminent botanists whose names are connected with these investigations which corrected and added to the splendid results of Hofmeister's pioneer efforts. Within the last few years a still further impetus has been given to this subject by the great improvements in histological methods which have made it possible to solve many problems which quite baffled the earlier botanists. This has been especially the case in the study of the so-called heterosporous Pteridophytes, where the development of the earlier stages goes on within the spore, but applies also to the exact study of all the delicate tissues. These improvements consisted first in the application of the careful methods employed by zoologists for preserving and staining tissues, and secondly the use of the microtome for sectioning delicate organs. Undoubtedly the name of Strasburger must lead the list of those who have been concerned with the improvement of histological methods.

The study of the Archegoniatæ has an especial interest,

as they undoubtedly are the connecting links between the Flowering Plants and the simpler green Algæ which most botanists are agreed are allied to the progenitors of all the higher plants. Just which of the modern forms are most nearly allied to this ancestral type is not certain, but on the whole the genus *Coleochæte* seems to show the greatest resemblance to the simplest known Archegoniatæ.

It is the purpose of this paper to sketch briefly the process, so far as we can trace it, by which the vascular plants gradually developed from the lower Archegoniatæ, and to state the somewhat conflicting views held by different botanists at the present time as to the relationships of the principal groups of the Archegoniatæ to each other, and their connection with the Algæ on the one hand, and the Spermaphytes (Phanerogams) on the other.

In all of the Archegoniatæ there is a marked alternation of sexual and non-sexual plants which are extremely different from each other both in structure and development. In the lower members of the series, the non-sexual generation (Sporophyte) is insignificant, and physiologically is merely a spore-fruit borne upon the sexual plant or gametophyte, but in the higher ones it gradually becomes more and more an independent organism, and finally all connection with the gametophyte is severed.

The Archegoniatæ are usually divided into two groups, the Muscineæ or Mosses in the widest sense, and Pteridophyta or the Ferns and their allies. In the first series the sporophyte never becomes entirely independent; in the second it always does. Within the first, however, there is great difference in the degree of dependence of the sporophyte. In the simplest ones, *e.g.*, *Riccia*, the sporophyte is simply a mass of spores surrounded by an imperfect layer of sterile cells. In all the others, however, a greater or less amount of sterile tissue is developed, which at first serves simply to nourish the growing spores, but in the higher Mosses and Liverworts forms a true assimilative tissue, with green parenchyma and stomata quite comparable to the assimilative tissue system of the vascular plants. The resemblance is still further increased by the

presence of an axial mass of tissue (columella) which both morphologically and physiologically is probably to be directly compared to the vascular bundles of the higher plants. In the sporophyte (sporogonium) of these higher Muscineæ, only a very small amount of tissue is devoted to the production of spores, in strong contrast to the lower Hepaticæ where practically the whole sporogonium is composed of sporogenous cells.

While in a general way it may be said that the evolution of gametophyte and sporophyte in the Muscineæ is parallel, this is by no means always the case. Thus *Anthoceros*, whose sporophyte comes the nearest to an absolutely independent condition of any Bryophyte, has the gametophyte of an exceedingly simple type. In this genus, the sporophyte not only possesses an elaborate assimilative system and a central conducting tissue, but its growth is not stopped by the formation of spores, but continues as long as the gametophyte remains alive.

In the Ferns the development of the sporophyte is carried still further, and by the production of a special organ, the root, it becomes entirely self-supporting and sooner or later the gametophyte dies. The sporophyte of the Fern at first sight shows little in common with the sporogonium of the most highly organised Moss, but a more careful scrutiny shows that the differences are less than appears at first. The great difference consists in the development of special organs, leaf, root, stem, sporangia, in the former, while the compact bryophytic sporogonium in no case shows any such external differentiation.

On comparing the gametophyte of the Pteridophytes with that of the Muscineæ we find that it resembles in most cases the simpler Hepaticæ rather than the more specialised Mosses; and in the case of most Ferns this resemblance is extraordinarily close. The earlier observers noticed the very obvious resemblance of a Fern prothallium to a thallose Liverwort, and concluded that here was the point of contact between the two series of the Archegoniatæ. When, however, an attempt was made to homologise the sporogonium of any known Bryophyte with the Fern plant, the

difficulty seemed insurmountable, and more recently another theory was advanced, of which Goebel is the principal exponent at present. This theory holds that the origin of the Pteridophytes must be sought very low down in the Archegoniate series before they had assumed the characters of the modern Muscineæ, and that the two series, Muscineæ and Pteridophyta, as they exist at present, form two entirely divergent lines of development arising from common ancestors, that in structure were intermediate between the green Algæ and the simplest Muscineæ. This theory is mainly based upon the alga-like prothallium of certain species of *Trichomanes*, a genus of the Hymenophyllaceæ. The latter are Ferns, mostly of small size and extreme delicacy, mainly tropical in their distribution. In the smaller species the very delicate filmy leaves remind one strongly of the leaves of some of the larger Mosses, such as *Muium*, and this resemblance led the older botanists to suggest a possible relationship between them, and later investigators have also been led by the apparent simplicity of their structure to assume that the Hymenophyllaceæ are the most primitive of existing vascular plants.

Goebel pointed out the resemblance between the prothallium of *Trichomanes* and the filamentous protonema of most Mosses, and from a study of the former, as well as that of some of the true Mosses, especially the peculiar genus *Buxbaumia*, came to the conclusion that the primitive form from which the other Archegoniatæ have sprung was a branching alga-like filamentous form, bearing the sexual organs directly upon the filament, and that the flat thallus of the Liverworts and most Fern prothallia is a secondary development, the two having nothing in common. Unfortunately Goebel does not offer any explanation of the origin of the sporophyte of the Hymenophyllaceæ either from forms such as exist at present or from hypothetical ones, but confines his attention almost exclusively to the sexual generation. This one-sided view of the question weakens very much the force of his arguments, and serious objections to his theory arise as soon as the subject is considered from other directions. Even putting aside the

structure of the sporophyte, the comparison of the filamentous prothallium of *Trichomanes* with the protonema of a Moss is hardly convincing, as the former is homologous not with the protonema alone, but with the protonema *plus* the leafy stem, and the Moss *Buxbaumia*, which he considers the most primitive in that the leafy stem is extremely reduced, is known to be saprophytic, and this abnormal habit is quite sufficient to account for the great reduction of the vegetative organs of the gametophyte.

While the filamentous prothallium of some species of *Trichomanes* is certainly strikingly like a Moss protonema, other species have the flat prothallium like that of other Ferns, and this is always the case with normally developed prothallia of all species of the closely-relative genus *Hymenophyllum*. These differences might be explained if we had to consider only the prothallium, but when in addition the character of the sporophyte is taken into account it becomes impossible to reconcile the radical differences between the simplest forms of the Hymenophyllaceæ and any known Bryophyte.

The forms of the Bryophytes whose protonemata most resemble the prothallium of *Trichomanes* are all specialised types whose sporogonia are very far removed indeed from the simple character that would be expected in primitive forms, while all the Bryophytes whose sporogonia can in any way be considered primitive are thallose, and much more like the flat prothallium of the majority of Ferns; this, in connection with the great similarity in the method of growth of the two, makes it seem much more likely that the older view of the origin of the Ferns from forms like the thallose Hepaticæ is correct. This seems the more probable as a study of the sexual organs shows much in common between some of the thallose Liverworts and certain Ferns, while all the true Mosses differ much from the latter in this respect.

Botanists at present usually divide the Ferns into two main divisions, the Eusporangiatæ and the Leptosporangiatæ, the distinction being based upon the character of the sporangia. In the latter group, which comprises an

enormous majority of all existing Pteridophytes, the sporangia are derived from a single epidermal cell, and in all cases provided with a curious ring of thickened cells, the annulus. In the Eusporangiatæ the sporangia are much more massive and multicellular from the first, and the annulus is extremely rudimentary or usually entirely absent. The latter group is unquestionably nearer to the seed plants in the structure of the sporangium, and this has been the cause of their being regarded as higher in the series than the Leptosporangiatæ which are connected with them by almost insensible gradations ; and it is only quite recently that it has been suggested that the Eusporangiatæ, while doubtless related on the one hand more nearly to the flowering plants, are at the same time nearest the Bryophytes. This hypothesis considers the Leptosporangiatæ as a comparatively modern group of plants, derived from the Eusporangiatæ, which are also the ancestors of a portion, at least, of the seed plants. If this view is correct the Eusporangiatæ are an ancient generalised type connecting the Bryophytes directly with the higher vascular plants, and from them as a starting-point have diverged two, and possibly more, distinct lines of development, one of which has terminated in the most specialised of the Leptosporangiatæ, and another in the Angiosperms. According to this theory the Hymenophyllaceæ, instead of representing the most primitive group of vascular plants, are really a somewhat specialised one, whose peculiarities are largely the result of adaptation to a peculiar environment, all of them being inhabitants of dark moist localities to which their delicate structure is especially adapted. In their essential characters they show most evidently a close relationship with the other Leptosporangiatæ and no suggestion of structure comparable to that of the Bryophytes.

The more carefully the Eusporangiatæ are examined, the stronger becomes the conviction that they are really primitive forms connected with the higher Bryophytes. This is true both as regards the prothallium and the sporophyte. The former reaches among these a degree of

development not found elsewhere, and resembles in a most remarkable degree that of many thallose Liverworts. This is especially the case with the sexual organs which are much like those of *Anthoceros*, whose sporogonium has long been recognised as the nearest in structure among the Bryophytes to the Ferns. This resemblance of the sporophyte in the two, is more than a general one. Not only is the development of spores in *Anthoceros* subordinate to the vegetative growth of the Sporophyte, but the origin of the sporogenous tissue is directly comparable to that in the eusporangiate Pteridophytes. In both cases the sporogenous tissue (Archesporium) is derived from hypodermal cells, and the segregation of the sporogenous cells into definite sporangia is already indicated in the Anthoceroteæ, although the sterile cells do not form firm walls between the masses of spores. Of the living Ferns, *Ophioglossum* seems the nearest to *Anthoceros* so far as the sporophyte is concerned. The sporangial spike of the former may be directly compared to the upper part of the sporogonium of *Anthoceros*, from which it differs mainly in the complete separation of the archesporial cells, so that a number of separate cavities are formed, constituting the large sporangia of this genus. In some species, *i.e.*, *O. vulgatum*, the epidermis above the masses of spores develops stomata, as in *Anthoceros*, and as in that form the sporogenous cells are hypodermal and the outer wall of the sporangium at first but a single layer of cells, which later becomes much thicker. The sporangia project very little, and compared with other Ferns are large. The genus *Botrychium* contains a number of species by which *Ophioglossum* is connected with the *Leptosporangiatæ*. There is a gradual increase in the definiteness of the sporangium, which becomes more and more raised above the surface of the leaf until in the more specialised forms, *e.g.*, *Botrychium Virginianum*, it is a globular capsule raised upon a short stalk. As the sporangium becomes more distinctly separated from the leaf, it becomes smaller, with a corresponding increase in the number of individual sporangia. Thus while in *Ophioglossum* the fertile leaf segment forms a

spike with only two rows of sporangia, in the larger species of *Botrychium* it is a compound panicle with very numerous, but very much smaller sporangia. This increase in the number of sporangia is correlated with a corresponding division of the sterile segment by the leaf which becomes very ample and much divided.

The type of sporangium in *Botrychium* is very much like that of the Osmundaceæ, which in turn are closely connected with the true leptosporangiate Ferns; but whether there is any genetic relation between *Botrychium* and *Osmunda* is another question, which at present we are not in a position to answer. In these forms, as well as in all the typical Leptosporangiatæ, the archesporium is readily traceable to a single cell, and in the latter group the whole sporangium is derived from a single epidermal cell, which is never the case in the Eusporangiatæ. In all the Leptosporangiatæ the wall of the sporangium is reduced to a single cell in thickness, and is usually long-stalked, especially in the Polypodiaceæ, which are probably the highest of the homosporous Ferns.

The very complete series of forms leading from the Leptosporangiatæ to the Eusporangiatæ has often enough been commented upon, but until the recent attempts to prove the primitive character of the latter it was taken as a matter of course that the Leptosporangiatæ formed the beginning of the series, and not the end. The objections to this view may be stated briefly as follows. First, the type of sporangium of the Leptosporangiatæ is very specialised, while that of the Eusporangiatæ approaches the condition found among the Bryophytes; second, the prothallium of the Eusporangiatæ approaches much nearer in structure the thallus of the Hepaticæ, both in its growth and in the structure of the sexual organs, and the embryo is much longer dependent upon it; thirdly, the geological record shows conclusively that the Eusporangiatæ were the prevailing forms among the Carboniferous and pre-Carboniferous Ferns, and the Leptosporangiatæ only appeared later, increasing in number and variety in the more recent strata.

The method by which a simple Fern like *Ophioglossum* may be supposed to have arisen from a structure like the sporogonium of *Anthoceros* would be somewhat as follows. From the large "foot" or absorbent organ at the base of the sporogonium of the latter, by further growth an organ might be formed piercing the thallus and penetrating into the ground. It is extremely probable that in some such way as this the first root of the vascular plants originated, and so soon as this took place the sporophyte would be quite independent of the gametophyte. The next step would be the development of a special assimilating organ, the leaf, which might be expected to arise as an outgrowth from the zone of active cells between the foot and the capsule in such a form as *Anthoceros*, which would then strikingly resemble a small *Ophioglossum*. The vascular bundles, which of course are absent, at least in their complete form, from the sporogonium of all Bryophytes, are undoubtedly directly comparable to the central strand of vegetative cells which forms the columella of *Anthoceros*, and is also found in the sporogonium of most Mosses. Even in these forms this central bundle of cells probably acts as a conducting tissue, although the different kinds of tissue found in the bundles of the true vascular plants are not yet differentiated.

In comparing *Anthoceros* directly with such Pteridophytes as *Ophioglossum*, it must not be supposed that a direct connection between the two is assumed. There are some differences so great as to forbid this. Thus the extremely peculiar chlorophyll bodies of all the Anthoceroteæ are quite confined to this order, and are very different from those of the Ferns. The spermatozoids, too, have but two cilia, instead of the numerous ones found in all true Ferns. However, in spite of these differences, the resemblances are so striking that it is safe to assume that the ancestor of the Pteridophytes was probably a liverwort-like plant having a sporogonium essentially like that of Anthoceros.

The latest theory as to the method of origin of the Pteridophytes is what may be called the "Strobiloid Theory".

This assumes that the forms like *Lycopodium* and *Equisetum*, in which the sporophyte forms a strobilus, or cone, are the most primitive. Professor Bower, who has advanced this theory, bases his conclusions upon a most thorough study of a very large number of forms, and his arguments are very convincing, at least so far as concerns the really strobiloid forms. In regard to the Ferns, there are some serious difficulties in the way. This theory assumes, as does the one just considered, that the ancestor of the Pteridophytes was a form like *Anthoceros*, with a continuous hypodermal archesporium, and that this in turn was derived from simpler forms in which the sporogenous tissue was much more prominent. The first step in the development of the strobilus was the segregation of the sporogenous cells by masses of sterile tissue, exactly as we have supposed in the evolution of *Ophioglossum*. Instead, however, of this sterile tissue simply forming the partitions between the sporangia, it is supposed to have developed into outgrowths or leaves, so that corresponding to each leaf there was a separate sporangium such as is found in *Lycopodium*. The form which Professor Bower considers to come nearest the primitive condition is a curious little lycopodinous plant, *Phylloglossum*. This interesting form shows certain striking resemblances to the embryo of various species of *Lycopodium*, the common club mosses, and seems to have retained its primitive or embryonic condition. From such a type as this the other Lycopodineæ are supposed to have descended.

Professor Bower is inclined to regard all of the Pteridophytes as reducible to a strobiloid form, but as far as the Ferns are concerned there are very serious difficulties in the way. Aside from apparently essential differences in the sporophyte between Ferns and Lycopods, there are also differences in the gametophyte which cannot readily be explained on the ground of a common origin. The most essential difference is in the spermatozoids. These in the Lycopodineæ are bi-ciliate like all known Muscineæ, while in the Ferns and Equiseta they are multi-ciliate. It is quite conceivable, nay, probable,

that there have been two distinct lines of development, perhaps more, among the Pteridophytes, arising quite independently of one another, but from similar ancestral forms. In one of these, the Lycopodineæ, the leaves are small, the axis much elongated, and regularly each sporophyte bears but a single sporangium. In the other, the Filicineæ, the leaves are large and the axis usually much less developed; sporangia are found usually in large numbers upon the, sometimes, profoundly modified sporophylls. Whether the third group of living Pteridophytes, the Equisetineæ, form a third entirely distinct series, or whether they are more nearly related to one or the other of the two other Classes, is by no means easy to determine.

The parallelisms in development found in the three groups do not necessarily imply a genetic connection, as perhaps the most remarkable case of all, that of heterospory, has beyond question arisen independently in at least five widely remote groups where the question of relationship cannot for a moment be maintained. The development of heterospory in unrelated groups of Pteridophytes also suggests the possible multiple origin of the "Flowering Plants," or, more correctly, "Seed Plants". That heterospory was the first step in this process no botanist would think of disputing, and the homologies of the macrosporangium of the Pteridophytes and the ovule of the Phanerogams are universally admitted, as well as those of the microspores and pollen cells. When, however, the question is raised as to which group of Pteridophytes has given rise to the Phanerogams, and whether all of the latter belong to a common stock, the matter is not so simple. Hofmeister first showed that the homologies between the Gymnosperms and the heterosporous Lycopodineæ were very striking, and that the former, except in the absence of spermatozoids, were really nearer to the Pteridophytes than to the Angiosperms. It was assumed that the latter were derived from the Pteridophytes through the Gymnosperms and that the Phanerogams formed a single series; but later many botanists have dissented from this view and are inclined to look for the origin of

the Angiosperms directly from some group of Pteridophytes. The peculiar genus *Isoëtes* has been suggested as a form possibly resembling this ancestral type, but it is still an open question to which group of homosporous Pteridophytes *Isoëtes* is most nearly related.

The essential points of structure of the Phanerogams, the seeds and pollen tube, are only further developments of heterospory, and may very readily be conceived to have arisen independently more than once. The structure of the Angiosperms is too constant to make it probable that the Monocotyledons and Dicotyledons are not intimately related, but among the Gymnosperms the diversities of structure are so great that it is quite possible that the Conifers and Cycads, for example, may not be at all related, but constitute two quite separate lines of development.

RECAPITULATION.

To sum up then what we have endeavoured to state in the foregoing pages, it is generally admitted that the origin of the vascular plants is to be sought among the less specialised Bryophytes, which in turn are unquestionably derived from algal ancestors. There is, however, diversity of opinion as to whether the first vascular plants came from forms similar to those existing at present, or from forms immediately between them and the Algæ.

It is further admitted that in the evolution of the sporophyte it gradually passed from a condition where its whole substance was devoted to spore-formation to that where more and more spore-formation was subordinated to the vegetative life of the sporophyte. This now, by the development of special organs, roots and leaves, became entirely free from the gametophyte upon which in the lower forms it had lived as a parasite. Of the living Bryophytes, or non-vascular Archegoniatæ, the genus *Anthoceros* comes nearest to realising this condition.

From a condition like that found in *Anthoceros* it is quite probable that more than one line of development has proceeded, resulting in the different groups of Pteridophytes, which in their turn may have given rise to seed-bearing plants quite independently of one another.

DOUGLAS HOUGHTON CAMPBELL.

RECENT RESEARCHES IN THERMAL METAMORPHISM.

PART II.

SOME interesting instances of metamorphism in various impure calcareous rocks (calc-mica-schists) have been described by Weinschenk from the Venediger *massif* in the East Central Alps (19). The igneous intrusions that have produced the transformations have been almost pure pyroxene rocks, and are now represented by serpentine rocks showing many features of interest. In contact with the serpentine at the falls of the Islitz is an "omphacite-like" rock consisting of tremolite, grey limestone, etc., with infiltration of serpentine substance. This zone is sometimes several metres in width, and is succeeded by a complex of crystalline silicate rocks, which sometimes recall eclogites and allied types, and are true lime-silicate-hornfels of various constitution. In part they are characterised by garnet, but nearer to the contact epidote and diopside occur instead, besides a blue amphibole and apparently a plagioclase felspar. The garnets often show double refraction and zonary growth, a character frequently found in the garnets of metamorphosed calcareous rocks in different areas. In other localities described by Weinschenk the metamorphosed calcareous schists have the character of "epidote-hornfels" (with plagioclase, epidote, and actinolite), "diopside-hornfels," etc. Whether these observations throw any light on the origin of eclogites and similar rocks may be doubted, since, as pointed out by Rosenbusch, the typical eclogites of Central Europe agree in chemical composition with true igneous rocks. It is certain, however, from observations in other districts, that the monoclinic pyroxenes produced in thermal metamorphism include omphacites, *i.e.*, varieties rich in alumina and lime, as well as the non-aluminous diopsides, etc.

As already intimated, under the conditions which obtain

during thermal metamorphism, the carbonates are not decomposed except in presence of silica in some available form to replace the carbonic acid. Consequently the most striking effects are to be observed in limestones or dolomite rocks containing a large percentage of impurities and in calcareous shales, tuffs, etc. Here the carbonic acid is entirely expelled, and rocks are formed consisting in great part or wholly of lime-bearing silicates. Such a rock—the Kalksilicathornfels of the Germans—is often of very complex constitution, and may be of so fine a texture as to require very minute study. Of this type are the compact porcellanous-looking rocks consisting essentially of a fine mosaic of augite, wollastonite, felspar, etc., sometimes enclosing rather larger crystal plates of some pyroxene, nests or patches of tremolite needles, etc. On the other hand there are lime-silicate rocks of which the constitution is more simple and manifest, such as the type which consists of dodecahedra of lime-garnet set in a matrix of crystalline idocrase. These and other types are found in various beds of the Coniston Limestone group, where they are metamorphosed by the Shap granite (3), and in these rocks English geologists may study almost all the phenomena that have been described in the classic districts of the Harz and the basin of Christiania.

A survey of the recorded facts regarding the lime-silicate rocks leads us to two or three general conclusions. In the first place we note that quite a moderate proportion of calcareous matter in a rock, which would not cause a field geologist to style it a limestone, is sufficient to make the metamorphism follow this line, and that such a rock, in contrast with a purer limestone, has its carbonic acid completely eliminated in the process of metamorphism. Further, we see that such changes in impure calcareous rocks are quite readily induced, the lime-bearing silicates apparently demanding no very elevated temperature for their production in this way : this fact seems to connect itself with the rather easy fusibility of the minerals in question, though the conditions of metamorphism are, of course, different from those of dry fusion. Lastly, the great variety in

mineral constitution of the rocks produced and the complexity of some of them are very striking. Calcareous beds which seem to differ but little may give rise by metamorphism to quite different mineral aggregates. Presumably slight differences in the chemical composition of the rocks affected may determine wide diversity in the recombinations set up in them, thus emphasising the prime importance of the bulk composition as affecting the results of the metamorphism.

It is only in recent years that any detailed study has been made of thermal metamorphism in *rocks of igneous origin*. The results promise to shed considerable light upon the problems offered by large areas of crystalline schists. It may be laid down as a broad rule that fresh igneous rocks are much less susceptible to changes by thermal metamorphism than are most ordinary sediments. This is a simple consequence of the fundamental principle of these mineralogical transformations, *viz.*, the instability of chemical compounds under changed physical conditions. Substances such as the common weathering products, formed at ordinary atmospheric temperatures, become unstable when subjected to much higher temperatures; but minerals which have originated from igneous fusion are, as a rule, much less liable to change when heated. In both categories there are a few exceptions, *e.g.*, quartz under the former head and the pyroxenes under the latter, but the rule is of general application.

Conversely, combinations entered into at high temperatures, as in rocks formed from fusion, cease to be stable at ordinary low temperatures; and most igneous rocks which are not geologically very young have suffered more or less change from this cause. The transformations induced in them are further connected with the access of meteoric water, carbonic acid, oxygen, etc., as is evident from the nature of the common "weathering" products. Igneous rocks so altered are quite as readily affected by thermal metamorphism as argillaceous or other sediments, the first signs of change appearing always in the decomposition products of the rocks.

The earliest observations of thermal metamorphism in igneous rocks were made by Allport (1876) on certain diabases near the Land's End granite, and similar phenomena have been described by Lossen in the Harz and by other geologists in several other districts. The chief features noted were the conversion of augite to green hornblende, and the recrystallisation of the felspar; and these are found everywhere to be highly characteristic changes. Brown mica may be formed with, or instead of, hornblende, but, in some cases at least, this seems to arise not directly from the augite but from its chloritic decomposition products. Other basic rocks show like transformations. The gabbro metamorphosed by the Carrock Fell granophyre in Cumberland has developed green actinolitic hornblende and brown biotite (20). The latter occurs chiefly around the grains of iron ore, from which it has probably taken up some ferrous oxide and titanic acid. At the same time the turbid felspars have become clear. Callaway (21) has described the metamorphism of diorites in the Malvern range and in Galway. In the former case the hornblende, perhaps previously weathered, has given rise to deep brown biotite in the vicinity of an intruded granite, while white mica has been produced from the plagioclase felspar. In the other case the plagioclase has been simply recrystallised, while a chloritic mineral, epidote, and rarely biotite, represent the original hornblende. In an altered diorite on the western flanks of Dartmoor McMahon (22) finds a red mica as the characteristic secondary mineral, most of the hornblende having disappeared.

On the metamorphism of volcanic rocks we have fuller information. Besides the cases already noted in Saxony, there are most instructive examples in the English Lake district, especially around the Shap granite, where lavas and tuffs of acid, intermediate, and basic composition may be studied in various stages of alteration (3). One general conclusion there brought out is confirmed elsewhere, *viz.*, that the acid rocks are commonly much less liable to metamorphism than the more basic. This is doubtless due to their more simple chemical composition. The rhyolites

close to the granite often show little or no change that can be certainly attributed to heat, and the rhyolite fragments in a highly altered calcareous breccia retain all their minute structures—cryptocrystalline, microspherulitic, and perlitic—in perfection. Where, however, the rocks before metamorphism contained decomposition products, these have given rise to new minerals, the characteristic green pinitoid substance being converted to a mixture of white and brown micas. In the acid tuffs, too, which were more composite and more weathered than the lavas, various metamorphic minerals have been set up.

The augite-andesites of the same district had been extensively weathered prior to metamorphism. Chloritic minerals, calcite, chalcedony, and quartz had been formed from the pyroxene and felspar, and were partly disseminated through the mass, but largely collected in little veins and in the abundant vesicles of the rocks. These weathering products were the parts most readily affected by heat. The chloritic mineral has been converted into biotite, or, where it was associated with calcite, into green hornblende, which specially characterises the metamorphosed amygdules: chalcedonic silica has been transformed into crystalline quartz. Nearer the granite the lavas are more altered, sphene, magnetite, pyrites, etc., being formed; the porphyritic felspars are replaced by a mosaic of new felspar substance; and finally the whole mass of the rock is reconstituted into a fine-textured ground.

A more basic type of lava shows similar phenomena, but, in consequence of the higher percentage of lime present, the new-formed minerals are not quite the same. Green hornblende here predominates over biotite, and an augite has also been formed, especially in the amygdules and veins. Another characteristic mineral is epidote. Some large amygdules show a complex association of lime-bearing and other metamorphic minerals—garnet, hornblende and actinolite, augite, quartz, sphene—and, in the centre of the largest ones, some residual calcite recrystallised but not decomposed. Another type of basic lava—the well-known hypersthene-bearing rock of Eycott—is

metamorphosed by the Carrock Fell gabbro, the bastite pseudomorphs after hypersthene being converted into hornblende (20).

The basic and intermediate tuffs near the Shap granite behave in general very like the associated lavas, though with some differences. Brown biotite is always the most conspicuous mineral in the metamorphosed rocks, and gives them the character of mica-schists or fine micaceous gneisses. A marked foliation is usually evident, the mica flakes being developed parallel either to the bedding or the cleavage of the tuffs, and a similar foliation often follows the flow lines of the lavas. The metamorphism of some Carboniferous volcanic tuffs on Dartmoor has been described by McMahon, the chief point of interest being the production of anthophyllite, a mineral not yet recognised in the Shap district. It occurs in radiating bundles of needle-like crystals with the usual characters (22).

But little detailed information has been collected relative to thermal metamorphism in the various types of *crystalline schists* and allied rocks. Among contributions to this subject in recent years the late G. H. Williams' account of the metamorphosed rocks on the Hudson River (23) is of special interest. It illustrates the changes set up in a series of mica-schists invaded by massive intrusions of diorite. Approaching the intrusive rock, the mica-schists are found to become more and more contorted and filled with "eyes" or lenticles of quartz containing garnet and other "contact minerals"—a feature noticeable in the metamorphosed flags of the New Galloway district already mentioned. In the mica-schists themselves have been developed staurolite, sillimanite, cyanite, and garnet, the completeness of the metamorphism increasing in the direction of the contact, where the schistose character is wholly lost and the rocks become hard and massive. In the progressive stages of metamorphism observed a gradual disappearance of the quartz and muscovite of the schists is found to accompany the development of biotite, sillimanite, staurolite, and garnet. This points to a chemical as well as a mineralogical change in the composition of the rocks, and indeed analyses show

a decrease in the proportions of silica (from 63 to 40 per cent.) and of alkalies and an increase in the proportions of alumina and iron oxides. The author does not assume that the rocks analysed were before metamorphism of identical composition, but making allowance for original differences, he claims that there has been a loss of certain ingredients or an addition of others. It is clear, however, that such rocks as mica-schists are not suitable for a strict inquiry on this point: we require rocks in which an individual bed can be followed into the metamorphic aureole, so that the material studied may be fairly assumed to have been of uniform character before metamorphism. The same objection applies to the conclusions which Hawes at an earlier date drew from the metamorphosed mica-schists near the Albany granite.

Some curious observations were made by Williams upon inclusions of highly metamorphosed schist in the mass of the intrusive diorite. Of these some consist mainly of pleonaste and corundum, with some magnetite, biotite, and felspar; others are essentially of quartz grains with various accessory minerals; others of staurolite and ripidolite with more or less sillimanite; and others again largely of flakes of margarite with varying amounts of other minerals. The phenomena bear a close resemblance to those seen in the neighbourhood of Klausen in the Tyrol, and the remarkable mineral aggregates constituting the inclusions are also paralleled by the included patches in the kersantite of Michaelstein in the Harz, described by Koch (24). The occurrence of minerals very rich in alumina, such as pleonaste and even corundum, is a curious feature in all the three districts.

An exhaustive memoir by Salomon deals with the thermal metamorphism of various crystalline schistose rocks in the Adamello range, between Italy and the Tyrol (25). The intruded rock which causes these transformations is the well-known "tonalite". Certain phyllites in the earlier stage of metamorphism show especially the production of brown biotite; nearer to the contact they pass into a schistose andalusite-mica rock consisting of muscovite,

quartz and biotite, with scattered crystals of andalusite and in certain layers blue grains of corundum. The intrusive rock itself contains inclusions, some similar to the neighbouring metamorphosed schists, others different and especially rich in cordierite.

More remarkable is the metamorphism of a lower group of gneisses, mica-schists, etc. The minerals composing the altered rocks are cordierite, andalusite, biotite, muscovite, quartz, and felspars, with sometimes garnet, tourmaline, sillimanite, spinel, corundum, and other minerals. Some of these are original constituents of the rock, such as ordinary orthoclase and muscovite, which disappear with further metamorphism; the rest are of secondary origin. In the inner zone of alteration, extending 100 or 150 metres from the contact, the rocks have lost all gneissic and schistose structures. The dominant type contains 60 or 70 per cent. of cordierite with biotite, andalusite, quartz, sillimanite, ilmenite, zircon, etc. The tonalite itself has inclusions which are taken to represent partially melted fragments of the adjacent rocks. They are rich in cordierite, but contain in addition spinel and corundum.

Phenomena more or less resembling those described by Salomon have been noted by Beck and Schalch in the gneisses and mica-schists of Saxony (26). The metamorphism of mica-schists has also been described by Müller from Schneekoppe, the highest mountain of the Riesengebirge, where the rocks abut upon an intrusive mass of granite (27). Here andalusite is the most conspicuous new mineral formed, in prisms several centimetres in length. The filmy layers of pale potash-mica of the original schists gives place to a dark magnesian variety in isolated flakes, the quartz occurs in isolated grains instead of in lenticles, and the schistose structure of the rock becomes much less marked. Further, the imperfect crystals of garnet full of inclusions are replaced by clean lustrous crystals with good forms. The metamorphism here is clearly much less intense than in the district surrounding the great tonalite intrusion of the Adamello Mountains.

It will be noticed that the most extreme metamorphism

is usually found in portions of the "country" rocks caught up as *inclusions* by intruded magmas, and the reason of this is easily understood. Moreover, in these included patches, if anywhere, we may look for evidence of actual permeation by the igneous magma, and consequent change in the bulk-composition of the fragments affected. Unfortunately in such cases we cannot always have certain knowledge of the original nature of the included rock before its transformation. Among a number of descriptions of such inclusions we may note that given by K. Vogelsang of the curious patches enclosed in the andesite of the Bocksberg and of Rengersfeld in the Upper Eifel (28). The constituent minerals are cordierite in polysynthetic crystals, andalusite in good prisms, sillimanite needles, felspar (mostly triclinic) in very variable amount, flakes of biotite, octahedra of green pleonaste, tabular crystals of corundum, etc. Very various associations of these minerals occur, and the relation of the inclusion to the matrix also varies. The general resemblance of these inclusions to what has been observed in the thermal metamorphism of some crystalline schists is evident, the abundance of aluminous minerals being again very striking. For instance, one inclusion, composed largely of sillimanite, gave about $47\frac{1}{2}$ per cent. of alumina. Precisely similar inclusions occur in the andesite of the Wolkenburg and the trachyte of the Drachenfels in the Siebengebirge, while some of the volcanic ejectamenta of the Laacher See also present many points of resemblance. Vogelsang gives reason for regarding them all as fragments of crystalline schists caught up in the igneous magma, which has partially melted them and given rise to new minerals. Some of the cordierite and the pleonaste are certainly newly formed, and the author has artificially imitated the phenomena by placing a fragment of cordierite-gneiss in melted andesite.

By a careful study of the inclusions in certain Scandinavian diabases, Bäckström (29) has succeeded in making out something of the manner in which they have been modified by the enveloping magma. This case differs, however, from the others mentioned above in that the

enclosed fragments were originally of granite. The ferromagnesian minerals of this rock, mica and hornblende, have been dissolved and absorbed by the diabase magma, rarely leaving any trace. The felspar crystals, chiefly microcline, have been attacked at the edges and penetrated along countless small cracks, which gave the basic magma access to the interior, where hollows have been dissolved out. The quartz grains of the granite have been only externally corroded and are thus rounded. The magma has given rise within and around the old microcline crystals to prisms of an intermediate soda-lime-felspar, in parts intergrown with quartz, and the effects produced may be likened in some respects to those described in the large felspar crystals of the Shap granite where they are enclosed in the dark and relatively basic portions of that rock (3).

The permeation or injection of a rock by an igneous magma, which we are here asked to consider, is clearly something more than mere thermal metamorphism, and must be essentially a *contact* action. It seems doubtful whether it has operated, except on a very restricted scale, at any ordinary junction of a metamorphosed rock with an intruded magma. Sollas has described it at the contact of a gabbro with a later intrusion of granophyre in the Carlingford district (30). The latter rock has penetrated the former in dykes and veins of all gradations of size down to microscopic films and specks occupying minute cracks and cavities in the gabbro, and the author seems to be of opinion that quartz-bearing gabbros in general have originated in a similar fashion. At Carrock Fell, however, where somewhat similar relations have been investigated by the present writer, such a process of injection seems to have been confined to a very narrow zone along the actual junction of the gabbro and granophyre, while the main body of quartz-gabbro owes its origin to a special kind of differentiation in the gabbro magma while still fluid (20). In the Irish case the details given of the extent and mode of occurrence of the rock types described are scarcely enough to warrant any expression of opinion on this point. Sollas has also described, in County Wicklow, the transformation

of a hornblende rock (amphibolite) into a rock having the mineralogical constitution of a quartz-mica-diorite. Traced from east to west, the rock undergoes a remarkable change, quartz and orthoclase as well as plagioclase-felspar being developed, while simultaneously the hornblende becomes actinolitic, and gives rise to much black mica. This change is ascribed to numerous veins of quartz and potash-felspar, which invade the rock on its western side.

The question, to what extent an actual *addition of material* may take place from an intruded igneous magma to the adjacent rocks metamorphosed by it, is one that has been much debated. Although the actual fact of such transference seems to be established in certain particular cases, the conditions which admit of it, and which limit its operation, are still very obscure. The commonest case is that in which the metamorphosed rocks near the junction have received an accession of certain *volatile* constituents, particularly boric and hydrofluoric acids. The characteristic minerals produced are tourmaline, topaz, axinite, fluorite, certain varieties of white mica, etc., and these minerals may occur in the marginal part of the intrusive rock as well as in the metamorphosed products. Such phenomena are well known in many districts, and seem to be specially connected with intrusions of certain thoroughly acid rocks: the Cornish granites afford good examples.

The difficulty arises, however, in connection with substances supposed to have been introduced *in solution* from an intruded magma and to have impregnated the adjacent rocks. The "contact" rocks known as adinoles in the Harz and other districts apparently give evidence of a notable addition of soda and silica. The rocks bordering Monzoni augite-syenites also show an addition of material, and other cases might be cited. On the other hand, the mica-schists described by Williams at Peekskill on the Hudson River seem to show a falling off of silica and alkalies as the junction is approached (23). An apparent falling off in certain chemical constituents may, of course, conceivably be caused by an increased proportion of other constituents of the rock. We naturally look, however, for

some relation between the particular constituents added to the metamorphosed rocks and the nature of the magma that furnished such constituents, and here the facts seem very difficult of explanation. The observed enrichment in silica and alkali is found not near intrusive rocks rich in those constituents, but especially in contact with diabases.

Brögger, as a result of his earlier researches in the Christiania district, laid down the rule that the phenomena of metamorphism are of the same kind, whatever the nature of the intrusive rock that has produced them. Recently he has found cause to modify this conclusion (31). At Sölvsberget, in the neighbourhood of Gran, the argillaceous shales with Ogygia are metamorphosed by a considerable intrusion of a basic rock described as olivine-gabbro-diabase. They are altered first into dark violet "hornstones" with increasing development of minute flakes of mica, and then into a visibly crystalline rock enclosing crystals of plagioclase-felspar more than 5 cm. in length. Near its contact with the basic intrusion this highly altered rock contains a considerable amount of hypersthene, and the magnesia and ferrous oxide of this mineral are supposed to have been obtained from the intruded magma. The same shales where metamorphosed by a more acid rock—the nordmarkite or quartz-syenite of the district—do not contain the exceptional mineral in question. The author writes guardedly, and the evidence is clearly incomplete without bulk analyses of the altered and unaltered shales.

Another question of considerable interest is that of the *interchange of material* within the mass of a rock undergoing thermal metamorphism, apart from any accession of matter from without (32). The very variable characters of many metamorphosed rocks and the preservation of such structures as the spherulitic, the amygdaloidal, and the laminated seem to indicate that any redistribution of material in the rocks during metamorphism is confined to very narrow limits. In other words the new minerals produced at any point seem to depend on the original chemical composition of the rock in the immediate neighbourhood of that point. In this connection the metamorphosed amyg-

daloidal lavas near the Shap granite are especially instructive (3), (32). These contain large amygdules, up to two inches or more in length, which before metamorphism were filled by decomposition products, including calcite, a chloritic mineral, and quartz. The calcite seems in general to have occupied the central part of the vesicle. As already mentioned, these substances have entered into new mineral combinations, among which lime-bearing silicates figure prominently; but in the centre there remains some calcite undecomposed. The manner in which this mineral is penetrated by needles of actinolite, etc., proves that it must have recrystallised during the metamorphism, and clearly its temperature must have been the same as that of other calcite in the same vesicle, which has been destroyed to form garnet, augite, etc. We have already remarked that in thermal metamorphism calcite is decomposed only in the presence of available silica to replace the carbonic acid driven off, and we now see that silica does not travel for this purpose more than a very small fraction of an inch. Results quite confirmatory of this extremely narrow limit to the migration of material are obtained from a study of calcareous nodules in metamorphosed shales, of the "spots" in altered slates, and of the dimensions of individual crystals of various metamorphic minerals. Whatever part water may play as a solvent in thermal metamorphism, it apparently does not act to any appreciable extent as a carrier to facilitate interchange of material between different parts of a rock.

In the opinion of the present writer the idea of solution can only be entertained in this limited sense, the action of any solvent or flux supposed to be involved being in the strictest sense local. We have hitherto discussed no speculations as to the processes by which new mineral combinations are set up during thermal metamorphism, and crystals of considerable size formed in the heart of what might be regarded as a solid rock. The question is one to which investigation has scarcely been directed by most inquirers. Hutchings (9) has recently approached it, his point of view being such as leads him to lay stress on the solvent action

of water at high temperatures, and to regard the new-formed minerals as recrystallised from such solution. He has made a special study of the amorphous matter, without action on polarised light, which occurs in many of the metamorphosed slates, etc., examined by him, and often plays an important part in such "spots" as do not consist of definitely individualised crystals. He finds gradations from the perfectly isotropic matter in question, through indefinite cryptocrystalline aggregates, into finely divided mica with other substances not certainly recognisable. The isotropic substance he believes to represent the result of relatively rapid cooling of the supposed solutions, while definite crystallised minerals represent, at the other extreme, the result of slow cooling. Further he conceives such solutions formed in the inner and intensely hot zone of metamorphism to be capable of permeating the outer and cooler zones and depositing there the dissolved material. In this particular his views are at variance with some of the conclusions which seem to follow from the observations of others. The conception of bodily solution and recrystallisation going on in a rock mass, which nevertheless retains not only its form but many of its minor structural characters, is one which confronts us with obvious difficulties, and we must be content to await further enlightenment as to the *rationale* of the complex processes involved in the mineralogical transformations of rock masses.

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ALFRED HARKER.

CONTINUOUS-CURRENT DYNAMOS.

Part II.

IN the preceding article, the question of the influence of the current in the armature of a dynamo upon its magnetic field was briefly discussed, and it was shown that later theorists had verified and expanded the original treatment of the subject by the Drs. Hopkinson in their paper on "Dynamo-Electric Machinery". Again we have to refer to the same paper as containing the germ of a recent remarkable invention, an account of which is reserved for the present article.

In 1886, the Drs. Hopkinson described an experiment, in which a machine was run as a generator without any use of its magnet winding in order to excite its field. The brushes were given a backward lead, *i.e.*, were shifted to the position, *no*, Fig. 3, which they would ordinarily

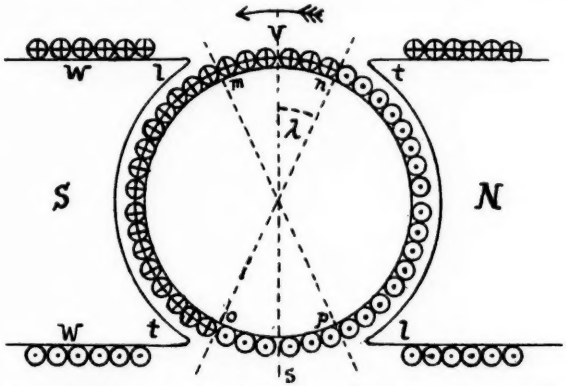


Fig. 3.—Forward and cross Ampère-turns of Armature with negative lead of brushes.

occupy in a motor; and when the circuit was closed, the residual magnetism of the iron caused a certain E. M. F. to be generated in the wires under the pole-pieces. A current was thus set up, and on comparing Fig. 3 with Fig. 2 of the last article, it will be seen that this current flowed through the wires between *mn* or *op* in the opposite

direction to that shown in Fig. 2. In this latter case their effect was to demagnetise the circuit, but now, of course, it must be the reverse. Thus the ampère-turns between *mn* and *op* in Fig. 3 have a forward magnetising effect, and increase the number of lines cut by the active inductors : or, in other words, the armature itself magnetises the field. This result sounds, perhaps, more remarkable than it really is. It must, of course, be remembered that when the brushes are placed at *no*, and the direction of the current is thus the opposite to that shown in Fig. 2 in the direct magnetising turns, any E. M. F. which is generated in these latter is *opposed to the direction of the current flowing in them.* The distribution of the field remains the same as in Fig. 2 ; as, however, the position of the brushes does not now coincide with the neutral line of zero field but is behind it, if the wires between the neutral line and the diameter of commutation are cutting any lines, they generate a *back* E. M. F. It is thus evident that no wire can at one and the same time act as a forward magnetising turn, and also as a useful inductor : the combination of the two functions is, in fact, entirely impossible. The forward magnetising loops, therefore, which Dr. Hopkinson obtained on the armature, in effect were but the translation of some of the usual stationary field-magnet winding on to the rotating armature. The wires themselves, it is true, move with the armature, but their place is continually taken by fresh wires, so that the current-sheets between *mn* or *op* remain stationary in space and magnetise the circuit. The account of the experiment ended with the following pregnant sentence : "If we could put up with the sparking that would ensue, it would be possible to make λ negative in a generator of electricity, and thereby obtain by the reactions of the armature itself all the results usually obtained by compound winding". This leads us to the method of winding armatures, recently invented by Mr. Sayers (Patent Nos. 16,572, 1891, and 10,298, 1893), and described by him in a paper read before the Institution of Electrical Engineers.[1]

[1] *Journal*, No. 107, vol. xxii., 1893.

Modifying the words of Dr. Hopkinson, we may say that experience has shown that we can *not* put up with the sparking which would ensue if the brushes of an ordinary dynamo were given a backward lead: but in the Sayers dynamo which we are now to consider, the sparking is *annulled* by the particular method of winding the connections between the loops on the armature and the segments of the commutator. Thus even though the brushes be given a negative lead in a generator of electricity, there is a position in which a sparkless collection of the current is possible. This achieved, the prediction of Dr. Hopkinson has found its fulfilment, and a dynamo has been produced in which, by utilisation of the reactions of the armature itself, all the results of compound winding have been obtained.

Let Fig. 4 represent the development on a flat surface of a portion of an armature, the short thick lines such as *bc* being the inductors on the surface of the armature connected together by the slanting portions such as *ab*, which may be either the internal inactive portions of the loops of a ring, or the end connections of a drum winding, in which latter case they will also include a second inductor on the diametrically opposite surface of the core. Each commutator connection, instead of being, as usual, taken straight from the point *a* or *c* to the nearest segment, is turned backwards against the direction of rotation for a short distance along the end of the core, and is then wound over the surface, parallel to and above the ordinary winding: it is then finally joined to the commutator segment in the usual manner as at A or C. The portion of the winding such as *c*C or *a*A is known as a reverser bar, or commutator coil. The brushes are then so placed that the reverser bars which at any moment lead to them are under or near to the trailing pole-tips.

An examination of Fig. 4 will show that of the three portions connected with the brush M, *viz.*, the short-circuited coil *abc* and the two reverser bars *a*A and *c*C, reverser C is moving in a stronger field and is generating a greater E. M. F. than reverser A, while the short-

circuited coil (marked by the open circle) is moving in the weakest field, being farthest from the pole-tip and nearest to the neutral line of zero field. The difference in the E. M. F. of the two reversers which at any time may be under a brush is indicated by the heavy black line of C, and the medium line of A: the other reversers, as shown by the thin lines, are entirely inactive, since they form no part of a closed circuit. Further, when a coil is short-circuited, a circuit *c*-*b*-*a*-A-C-*c* is closed by the contact of the brush surface; hence though the direction of the E. M. F. induced along the length of the two reversers is the same, the one opposes the other, and the greater E. M. F. of C as compared with that of A leaves an unbalanced E. M. F. acting round the closed circuit in the

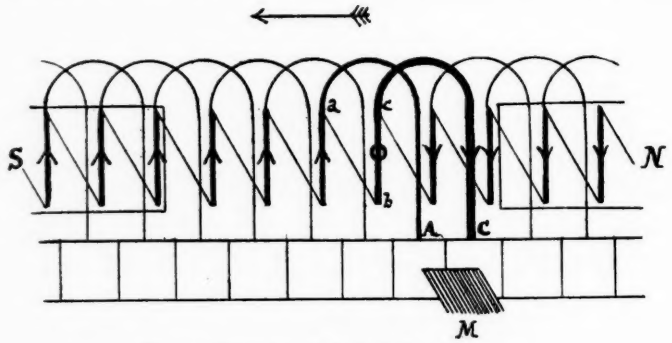


FIG. 4.—Sayers' method of winding Armatures.

direction *c*-C-A-*a*-*b*-*c*. Now the self-induction of section *abc* tends to keep the current flowing in its old direction, *viz.*, from *c* to *a*, this being the direction of the current in the loops to its right, as shown by the arrowheads on the inductors. But it has just been shown that the difference of the E. M. F.'s of C and A tends to drive a current through the short-circuited coil from *a* to *c*. If therefore this unbalanced difference of E. M. F. is sufficiently great, and is given sufficient time, it will stop the old current flowing from *c* to *a*, and will start a reversed current in the required new direction: at the moment when this new current reaches the strength of that which is being carried by the rest of the winding, segment A should pass from

under the brush and the short circuit will be opened without any sparking.

The question then arises, how is sufficient difference of E. M. F. between C and A to be obtained, so that the required reversing action may be provided? The solution of this is to be found in the adoption of a toothed armature core, and a short air-space between the iron of the armature and the iron of the pole-pieces. Such a type of dynamo has other merits which have led to its being frequently tried,—primarily, of course, its great economy in the copper of the field winding, and the electrical energy required for its excitation. But in the long run, experience has so far told against it owing to the difficulty of preventing sparking at the brushes in dynamos of large output. Now, however, the short air-space fits in admirably with the device by which the sparkless reversal of the current is secured, and in fact is essential to it. Let the armature be toothed, and the winding be buried in the slots or tunnels between the teeth, so that a small air-space of, say, one-eighth of an inch may be obtained between the armature and the pole-pieces. Then, owing to the closeness of the iron polar surfaces, the strength of field will fall off quickly as we pass outwards beyond the pole-tips, and the fringe of lines extending into the core from their edges will vary rapidly from a great density to practically zero. If now the winding and position of brushes be as shown in Fig. 4, the coil *cba*, being some distance from the trailing tip of the pole N, will be inducing little or no E. M. F. and will therefore be in a suitable state for commutation. Reverser bar C, being well within the fringe of lines spreading from the trailing pole-tip, will be generating considerable E. M. F., while reverser bar A, being farther away from the pole-tip, will be cutting an appreciably weaker field. Thus by experimentally shifting the brushes, a position may be found for them such that the difference in the E. M. F. produced respectively by C and A is just so great as to reverse the current in *cba* and to bring it up to its normal strength in the new direction, within the time that the coil is short-circuited by the brush.

But now a further advantage of the above method of winding appears. Since the reversing E. M. F. is drawn from the fringe of field at the trailing pole-tip, and this portion of the magnetic field is strengthened by the reaction of the armature current as explained in the preceding article, might not this strengthening be arranged to automatically furnish the stronger E. M. F. which is required to reverse an increased current in the armature? This has in fact been done and Sayers-wound dynamos have been designed and built in which the brushes may be left in the same position from no load to full load without sparking. The attendant is thus entirely relieved from any necessity to shift his brushes as the load on the dynamo varies.

The possibilities of the Sayers-wound dynamo have not as yet been fully explored and delimited, but it certainly holds out great promise for the future, and its invention must be ranked as perhaps the most striking novelty of recent years. The advantages which it possesses may be briefly summed up as follows: the fixed position of the brushes for all loads, the possibility of eliminating armature reaction as one factor by which the output of an armature is limited, and a great saving in the weight of copper and iron required for a given output. This latter advantage is due not only to the conversion of the back ampère-turns of the armature into forward magnetising turns, but also to the fact that a small air-space becomes permissible even in machines delivering large currents. The Sayers method of winding has been applied to a compensator giving eight volts with a current of 400 ampères,[1] in which case there was no field winding at all: and more recently a direct-coupled dynamo of 80 kilowatts output at 420 revolutions per minute[2] has been built, the total weight of which does not exceed three tons. But a still more important case is that of eight large dynamos which have been installed at the chemical works of Mansbo in Sweden[3] for the recovery

[1] *Electrical Engineer*, 14th July, 1893.

[2] *Electrician*, 14th September, 1894.

[3] *Elektrotechnische Zeitschrift*, 6th September, 1894.

of chlorine salts by electrolysis: each of these has an output of 115 volts and 1200 ampères and is a six-pole machine with drum armature coupled directly to the horizontal shaft of a 220 horsepower turbine. In considering the extent to which the Sayers winding is likely to be adopted in the future, perhaps the most serious question which the dynamo designer has to face is that of the cross armature reaction in large machines. The air-space being small, the loops lying under the pole-pieces have a proportionately great cross-magnetising effect: the induction at the trailing pole-tip is thereby forced up to a high figure, and, if care be not exercised in the design, the iron may become saturated either at the trailing pole-tip or in the teeth of the armature core under it, so that the total number of lines is reduced. Hence in the dynamos which have so far been built on the Sayers principle, a double horseshoe field has been usually adopted, which enables each pole-piece to be split in half and an appreciable air-gap to be interposed in the cross-magnetising circuit, without interfering with the true path of the lines into the armature core.

Finally we come to the field of practice, and it is here that the advance of the last few years is most marked. As the use of the continuous current for lighting, transmission of power, traction, and electrolysis has steadily increased, so also have the dynamos for supplying it grown vastly not only in number but also in size. Ten years ago, the dynamos in general use were of 10 to 50 horsepower, but now a unit of 200 horsepower is common in British central electric light stations, while on the Continent 500 horsepower dynamos are frequently met with, and in several cases monsters of even 1000 or more horsepower are successfully employed. Their forerunners are to be found in the Edison "steam dynamo" nicknamed "Jumbo," of which the output was 900 ampères at a pressure of 105 volts, and the "Colossus" Brush machine, for which an output was claimed of 3200 ampères and 80 volts at 405 revolutions per minute. The former is worthy of especial note from its anticipation of modern central-station work in its being directly coupled to the crankshaft of a horizontal steam-

engine at the slow speed of 300 revolutions per minute. The Colossus machine was constructed for reducing aluminium ore in an electric furnace, and the requirement of very large currents for electric smelting by the Cowles or Hérault processes has called forth some of the largest continuous-current dynamos in existence. In 1888 we find the electro-metallurgical works at Lauffen-Neuhausen using a 300 horsepower turbine to drive at the slow speed of 180 revolutions per minute two six-pole Oerlikon dynamos, each supplying 6000 ampères at 20 volts, or 120 kilowatts: each of these dynamos in reality consisted of two armatures wound on the same core, a commutator with six sets of brushes being placed on either side of the machine. In the same year, the Cowles Aluminium Company had at work a two-pole dynamo giving the very large output of 60 volts and 5000 ampères at 380 revolutions per minute. More recently the Oerlikon Company have constructed large multipolar machines also for the smelting of aluminium ores at Neuhausen, in which the armature is mounted on the extended shaft of a turbine and revolves in a horizontal plane within a ring of poles pointing radially inwards. The latest (1893) of these give an output of 7500 ampères at 55 volts or 550 horsepower when running at 150 revolutions per minute: they have twenty-four poles and a huge commutator from which the current is collected by 120 brushes in twenty-four sets; the total weight of turbine and dynamo amounts to as much as twelve tons, and the turbine is arranged to exert an upward thrust so as to take a part of this weight off the footstep of the vertical shaft.

The comparative performance of machines in relation to their size is best estimated by the number of watts per revolution per minute which they respectively give, the speed at which any output is obtained being in fact an all-important item in their comparison. Judged by this test, the output of the Cowles dynamo alluded to above, *viz.*, 440 watts per revolution per minute, must be regarded as very large for a two-pole dynamo. Probably the largest continuous-current two-pole dynamos in existence are those

now in use at the Carnaby Street station of the St. James' and Pall Mall Electric Light Company, and of which mention has already been made as having been exhibited at the Naval Exhibition in London. These give an output of 1500 ampères at 120 volts or 180 kilowatts at a speed of 350 revolutions per minute, and experience has shown them to be most satisfactory, both as regards heating and sparklessness. Their output works out to over 520 watts per revolution per minute, and it is noteworthy that this is attained by the use of a treble-wound armature, *i.e.*, on each armature core there are three separate and distinct windings, their commutator segments occurring in regular succession one after the other, so that every fourth segment belongs to the same winding. Thus the current is collected by two sets of wide brushes resting on the joint commutator, and each set covering not less than three segments simultaneously. It would certainly appear that, by this device of winding the armature so that there are more than the two usual halves in parallel, a larger output may be obtained without trouble from sparking or heating, and that therefore the limit of output within which the two-pole dynamo with its single horseshoe field may be used is extended. Still there must come a point when recourse should be had to a multipolar design, although it is still an open question at what point this limit is reached. On the Continent, as has been already said, multipolar types have found more favour than at home and are more generally used for smaller outputs. In foreign central stations, the dynamos are usually of larger size than with us and are coupled to engines of the slow-speed double-acting class. In a large number of cases they belong to one or other of two types, manufactured respectively by the firms of Siemens & Halske, and Schuckert & Co. The former has internal radial magnets surrounded by a ring armature, this latter being built up of segmental plates and supported on bolts from a hub so as to overhang the magnet system. The Berlin central stations contain the largest examples of this type. At the Markgrafen Strasse station there are four such machines each of 400 horsepower at 80 revolu-

tions per minute, with ten poles and a commutator five feet in diameter. In the dynamos which the same firm has built more recently, a feature of interest is that the separate commutator is discarded, and the numerous sets of brushes bear directly on the peripheral surface of the ring winding; the outer portion of each loop is a copper piece wedge-shaped in section, and is mounted so that the entire number forms in effect a gigantic commutator. At the Spandauer Strasse station there are four engines, each of 1000 horse-power, driving two dynamos which are mounted at either end of the engine crankshaft: each dynamo has ten poles and gives 2000 ampères at 140 volts when running at 60 revolutions per minute, whence each combined set is capable of lighting as many as 7000 lamps of 8 candle-power: while at the Mauer Strasse and Schiffbaudamm stations, there are three and six equally large double steam dynamos. The above gives an idea of the size to which the demand for electric light in towns has raised the continuous-current dynamo. Machines of the same type as above described are also to be found at Copenhagen and Stockholm.

The Schuckert dynamo for central-station work is of an entirely different construction, but again is multipolar. It has a large ring armature of a flat discoidal shape, flanked on either side by numerous poles facing the armature core: opposite poles are of the same sign and in effect form a common polar surface. In recent machines, the armature is adjustable end-ways, so that it may be set exactly central between the poles on either side; all thrust or pull on the core, due to the fields on one side being stronger than those on the other, is thereby obviated. At Hanover there are triple expansion engines of 400 and 600 horsepower, each coupled directly to their dynamos: the armatures, owing to their large diameter of 10 ft. and their high peripheral speed, serve as flywheels: the commutators are of huge size, 6 ft. 6 in. in diameter, and have as many as 840 sections. At Düsseldorf the dynamos have 14 poles and give 400 volts and 1000 ampères at 90 revolutions per minute, and again the armatures are 10 ft. in

diameter with 6 ft. 6 in. commutator. Similar dynamos are in use at the central stations of Altona and Aix-la-Chapelle.

Space does not permit of more than a passing mention of recent progress in traction and transmission of power by continuous currents. For the latter purpose, where the distances are great, the alternating current with single- or double-phase generators and motors, has lately proved a serious rival, but up to the present, for traction, the continuous current reigns supreme, since the distances are usually not so great as to demand a higher pressure than about 500 volts. After the Bessbrook-Newry electric railway and the pioneer lines of Portrush and Blackpool, little was done for many years in England, and the advance was chiefly on the other side of the Atlantic. Once again, however, England has come to the front by the recent construction of the City and South London Electric Railway and the Liverpool Overhead Railway, both of which for size and successful working are, in many respects, unrivalled. In the former, the current is delivered by 225-kilowatt belt-driven dynamos at 500 volts pressure to electric locomotives each containing two 50 horsepower motors. In the latter, each car is furnished with a motor of 40 horsepower, which is built direct on the axle, and the dynamos are driven from horizontal engines by cotton ropes. At the Chicago Exhibition of 1893, the supposed requirements of the future in the direction of rapid municipal traction were exemplified on a large scale by the gigantic generator of 2000 horsepower, which was employed in connection with the Intramural Electric Railway. Nor can this estimate be regarded as much exaggerated, when we find that the West-end tramways of Boston, which form the largest electric system in the United States, are fed by six triple-expansion engines, each of about 1000 indicated horsepower driving by belt eighteen Thomson-Houston four-pole dynamos. Of a different class is the electric rack-railway which ascends Mont Salève near Geneva: here the power station is situated on the Arve, and is furnished with two turbines, the vertical shafts of which drive multipolar Thury dynamos yielding 275 ampères at 600 volts,

or 165 kilowatts, at the very slow speed of 45 revolutions per minute: the dynamos have twelve poles, and thirty-six carbon brushes are used to collect the current. The gradient reaches in places 25 per cent., and each car has two four-pole motors giving 30 horsepower at 600 revolutions per minute, their speed being reduced by double gearing to 50 revolutions of the wheels.[1]

In the transmission of power, properly so called, by continuous currents, much has been done since the pioneer installation by which, in 1886, 50 horsepower was transmitted a distance of five miles from Kriegstetten to Solothurn: in this case the three-wire system was employed with two generators and two motors each of 1000 volts, and the over-all efficiency of the plant was 75 per cent., a result which at that time was regarded as little short of marvellous. It was erected by the Oerlikon Company of Switzerland, and in the succeeding years the same company followed up their first venture by a number of installations of various horsepowers, large and small. Among these may be mentioned the transmission of 60 horsepower over five miles at Lake Lugano, and of 100 horsepower for one mile near Aichberg in the Tyrol with an efficiency of 80 per cent., also with a pressure of 1000 volts. In 1891 they completed the important transmission of 600 horsepower for a distance of 750 yards at Schaffhausen, where the Falls of the Rhine supply the power. Each turbine of 350 horsepower was connected by means of bevel wheels and ropes to a six-pole dynamo with an output of 624 volts and 330 ampères at 200 revolutions per minute. At the spinning mills, to which the power is transmitted, there are two small motors of 60 horsepower each and one large double motor giving 380 horsepower. A commercial efficiency at full load of 78 per cent. was guaranteed by the makers.[2] Finally mention must be made of the very important installation by which as much as 1000 horsepower is obtained from

[1] *Elektrotechnische Zeitschrift*, No. 21, p. 289, 1894.

[2] Kapp's Cantor lectures on *The Electric Transmission of Energy*, delivered before the Society of Arts, 1891.

reservoirs on the mountain river Gorzente and distributed to Genoa and the Polcevera Valley. The fall is divided into three portions, each of which has its own station: the system employed in all alike is the high-tension continuous current with the generators and the motors in series, and they therefore closely resemble one another in their main features. Each turbine of 140 horsepower is directly connected by Raffard insulating couplings to two Thury dynamos and runs at a speed of 475 revolutions per minute. The dynamos are six-pole machines with drum armatures, and are designed to give a constant current of 45 ampères with a maximum pressure of 1100 volts. They are coupled together in series so that the voltage at full load amounts to about 6000 volts per circuit, and owing to this high pressure the insulation of the windings has received great attention. The bedplates of the dynamos are insulated from the ground by means of porcelain insulators filled with rosin oil, and not only are the armature shafts insulated from the turbine by the coupling as mentioned, but, in addition, the actual iron core of the armature is insulated from the shaft itself.[1] From the above it may be seen wherein lies the disadvantage of the continuous current for the transmission of power over very long distances. The nature of the commutator of the dynamo does not permit of more than at the most about 2000 volts being generated in any one machine, and we are therefore compelled to couple several machines in series in order to obtain the high voltage which alone will render the transmission efficient. It is then only by the most careful precautions that the complete insulation of the system can be secured, since the windings of running machines form part of it; whereas with the alternating system it is easy to generate the electrical power at low pressures and subsequently to transform it up to high pressures by means of stationary transformers.

As might have been anticipated, the list of transmissions of power includes but few cases within the British Isles.

[1] *Journal Inst. El. Eng.*, No. 107, vol. xxii., 1893.

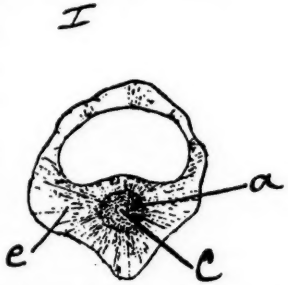
Owing to the scarcity of water power, little can be done in this direction at home, and schemes for the transmission of the energy of our coalfields to our towns by electrical stations at the pit's mouth remain but imaginative dreams, far removed from practical realisation.

C. C. HAWKINS.

ON THE MORPHOLOGICAL VALUE OF THE ATTRACTION-SPHERE.

PART II.

IN the previous part of this paper I drew attention to the spheres, originally discovered as structures composed of a large mass of protoplasm (archoplasm) containing a clear space and a central body (Fig. I.).



If we now take the centrosome in the figure (*c*) as a starting-point, it will be seen that each of the two succeeding zones of the sphere is concentrically disposed to the one internally preceding it. Moreover, this concentric zoning does not stop at the external boundary of the archoplasm (*a*) but is continued beyond it, in the shape of a more or less defined radiation of the general cell contents, which sometimes extends quite out to the periphery (*e*).

Such an external differentiated area of the protoplasm has not, however, generally been considered to be a part of the sphere, probably on account of its extremely fugitive and transitory character; nevertheless as there is little doubt that the radii (*e*) have sometimes been confounded with the archoplasm, in order to avoid a like confusion, I shall speak of them, when necessary, as the *radial envelope* of the sphere.

Through the researches of Flemming and others we have now abundant evidence of the existence of centrosomes and astral radiations in many kinds of tissue, in as many animal forms. But it becomes at once obvious, on instituting a comparison between their figures, or making preparations for oneself, that the usual appearance of the tissue (somatic) spheres is unlike the three-zoned structure originally described.

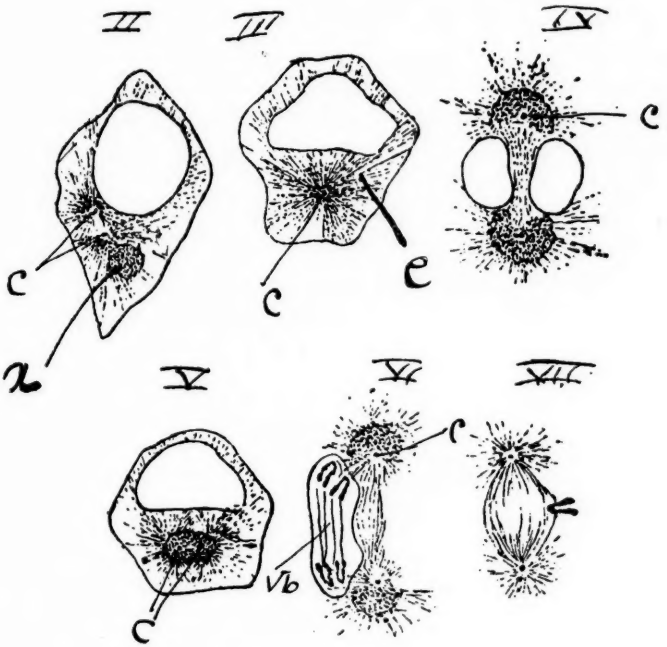
For example, in the cells of the amphibian lung and

mesentery, figured by Flemming, the sphere is composed of a centrosome, enclosed on the outside by the light space (*heller Hof* of the German authors) (Fig. III. *c*), from which spreads a more or less extended radiation through the general protoplasm of the cell. There is, however, nothing comparable to an archoplasmic differentiation of this substance, and in many cases (as for example in some leucocytes) these simple radii extend at once from the centrosomes to the cell periphery. They have in fact exactly the appearance of the radial envelope of a sphere of which the archoplasmic constituent is wanting (*cf.* Figs. I.-III.).

The spheres in these somatic tissues therefore appear to be less complex than those of the reproductive elements with which we have been previously dealing; they may be referred to a *simple* in contradistinction to a more *compound* type, and our appreciation of the mutual relationships of the different modifications in their structure, will, like other problems of comparative morphology, depend upon the possibility of finding intermediate forms.

So far, the compound spheres which we have examined have been such as exist either in ova, or some element of the ovigenetic series. It will be well therefore, for the sake of comparison, to ascertain what is known respecting the nature of these bodies in the analogous spermatic series of reproductive cells. In the spermatocytes of many, in fact I believe of all animals, whether vertebrate or invertebrate, which have been examined with sufficient care, there exists beside the nucleus a large slightly staining body, generally known in the earlier literature as the "Nebenkern". In the case of Salamandra and Proteus, this nebenkern was shown by F. Hermann (18) to contain a central granule, which, at the commencement of mitosis, divides into two halves, that separate from one another through the mass, and draw its substance out between them into an initial spindle figure, since known as *Hermann's Central Spindle.* The subsequent mitotic phenomena occur in relation to these central granules, as if they were true centrosomes, and consequently Hermann was justified in regarding the

nebenkern of the spermatocytes of Salamandra and Proteus as the archoplasmic portion of an attraction-sphere.



Since that time it has been observed that a similar *rôle* is played by the nebenkern in relation to the division of spermatic cells in a great variety of animal forms, and we are now probably justified in regarding the nebenkern as always equivalent to the *archoplasm* of those cells in which that structure was originally described.

When a cell with a compound sphere, like the spermatocyte of a salamander (Fig. V.), is going to divide, the archoplasm becomes so fully used up in the construction of the central spindle, that there is no such residual mass left round the centrosomes as in the mitotic figures of Ascaris (*cf.* Figs. VII., IV.). In this way the external radial envelope becomes directly related to the exposed half of each centrosome, in such a manner as to be quite indistinguishable from the protoplasmic radiations round the centrosomes of a simple sphere (Fig. VII.).

The succeeding events differ somewhat in different cells, as the spindle fibres sometimes, but not always, become equatorially centred to an *intermediate body*, in the division plane of the daughter elements.

Whatever be the exact course pursued, the greater part of the spindle fibres eventually fuse or coalesce to form an archoplasm in each new cell. Into these new archoplasms the centrosomes gradually find their way, and finally come to rest at the centre of each new mass.

Thus, during the course of such divisions, the archoplasm appears to be first modified into an initial spindle figure, and then into a complete one, the residual fibres of which coalesce to form two new archoplasms—one in each daughter cell. In this way the whole sphere has the appearance of persisting through successive cellular generations, archoplasm arising from archoplasm, centrosomes from centrosomes.

If, however, we now turn from the spermatocytes of Salamandra to those of the mammalia, as exhibited in a rat (Fig. II.), the persistence of the archoplasmic constituent in the former, turns out to be no sure or universal guide to its character as a cellular constituent; because, in these mammalian elements, although there is a large and obviously similar nebenkern or archoplasm in the first place, the centrosomes do not, even during rest, come to lie in its interior, but remain quite free in the cytoplasm, generally between the archoplasm and the nucleus (Fig. II. *c*). So it follows that the sphere is here normally dismembered or separated into two portions, one consisting of nothing but the archoplasm, and the other of nothing but the centrosomes. To these latter are centred the protoplasmic radiations, which, in the more ordinary type of sphere, would abut on the outer surface of the archoplasm, and form the exterior radial envelope of the sphere.

In the second place, when division approaches and the centrosomes separate from one another, a spindle is formed, partly from the radiation which originally surrounded these bodies, and probably also with the co-operation of some of the nucleoplasm (Fig. II.). Its whole course of develop-

ment, however, is absolutely independent of the archoplasmic structure, which degenerates, and during the later phases of mitosis vanishes in the cytoplasm altogether. The new archoplasms which arise in the daughter cells, as a coalescence of these cyto- and nucleo-plasmic spindle fibres, are therefore to be regarded as nothing but temporary modifications of the nuclear and cytoplasmic substances, and of an entirely non-persistent nature.

It will be seen that the simple radiation which spreads from the centrosomes in these dismembered spheres, gives to the former bodies exactly the appearance they possess in the tissue (somatic) cells whose spheres are of the simple type; while the concomitant presence of a functionless archoplasm converts the ensemble of cell structures into a form, with spheres intermediate between the simple and the compound types, such as we have been seeking, for it is scarcely possible to doubt that the simple radiation here present round the centrosomes (Fig. II.) is similar to that of simple spheres (Fig. III.). Nor can we resist the further inference that this radiation stands also for what would be an external envelope, were the centrosomes to enter into the archoplasmic substance.

From all this it would appear, then, that the radiation of a simple sphere (Fig. III. *e*) is really equal to the external radial envelope of the compound or reproductive type (Fig. I. *e*), and that in the former the archoplasmic constituent has become (gradually ?) suppressed and is now completely wanting. The facts, however, may be recapitulated as follows.

In the first (compound and probably primitive) type of sphere, the archoplasm persists, being alternately stretched out into a spindle figure and then in part recondensed in each succeeding generation. In the second it is not persistent, because, although it arises as a coalescence of the spindle fibres, owing to the extra-archoplasmic position of the centrosomes, it is not reconstructed into a spindle and degenerates in the cell. While in the third or simple type, the spindle fibres do not coalesce to form an archoplasm at all.

Hitherto these differences in the structure of the spheres have been implicitly, if not explicitly, regarded as indications of an indefinite variation in these organs between cell and cell, but if the facts be grouped together (as above) it becomes quite clear that there is some sort of method underlying the apparent meaningless variety of sphere formation, and that the modifications of these structures have probably as real a morphological significance as any grosser features of anatomy. At the same time, however, the fact should not be lost sight of, that, although we may thus split up a cell into zones having different potential destinies with respect to the structures which arise out of them, it does not follow that similar structures always arise from similar cellular zones. In fact this capacity of forming similar things from different sources which cells possess, constitutes an admirable example of the truth contained in Whitman's aphorism, "Organisation precedes cell formation".[1]

It is thus sufficiently apparent, from existing observations on the constitution of the spheres in tissue cells, firstly, that the archoplasm is not always present in them, and secondly, that where this structure does exist it is not necessarily persistent through successive cellular generations. It consequently follows, that the only portion of the sphere which can still lay any claim to persistence, or to a place

[1] The tendency apparent throughout the earlier literature, to regard the nebenkern as a sort of transmissible entity, has probably been the unconscious outcome of the fact that the earlier investigations were almost exclusively confined to cells in which the sphere is compound, and in which consequently there is an archoplasmic spindle formation.

There is perhaps nothing anomalous in the apparent indifference with which the spindle originates either as an intra- or an extra-nuclear structure, for it seems to me merely one mode of expressing what appears to be a fact, *i.e.*, that the kinoplasm (archoplasm) can take up either an intra- or extra-nuclear position. Nor is the dismembered condition of the sphere, to which I have alluded, at all an isolated peculiarity, as Lauterborn's figures of the separated centrosomes and archoplasm in diatoms will show (24). As Ischikawa very justly remarks, with the rather marked exception of the particular animal (*Noctiluca*) with which he deals, R. Hertwig's supposition (25) that the kinoplasm (archoplasm) of the Protozoa is intra-nuclear while that of the Metazoa is extra-nuclear, appears to have some foundation in fact.

among the prime factors of the cell, is reduced to the narrow limit of the centrosome. "Seine Grosse ist an der Grenze des eben sichtbaren, und bleibt häufig unter dem Durchmesser kleinster Mikro-organismen Zurück," as Hertwig says (26), while these minute constructions are, as we have seen, not in themselves distinguishable from other cellular granulations (microsomes). It is only their position in relation to the rest of the sphere, which confers upon these particles their peculiar distinction, and it is accordingly not surprising that within the last few years the opinion has continually gained ground among observers, that it is possible, if not probable, the centrosomes and microsomes may eventually prove to be really of the self-same nature. This view has found its most complete expression in the theory of Watase, who regards (27) the centrosomes as nothing more than accentuated microsomes, or rather cyto-microsomes as he prefers to call them. He consequently denies them the possession of any properties peculiar to themselves. They only exhibit those which might be acquired by any other cyto-microsomes, their actual peculiarities arising entirely by virtue of position; and, thus considered, they may be looked upon as structures lying in the foci of the forces which divide the cell.

The observations of Heidenhain (8) on the centrosomes in various forms of leucocytes have shown that they may become split up into one, two, three, or four small particles, which in their totality constitute what he terms a *Micro-centrum*, and his investigations of the giant cells of the red marrow show a still further multiplication of these central constituents of the sphere. He maintains, moreover, that the centrosomes grow by a process of continual budding, and that in this fact lies the explanation of the curious and habitual difference in size which they exhibit when paired, either during rest or at the apices of the spindle figure, the smaller being regarded by him as simply the younger of the two. Now, the cytosomes which exhibit this singular capacity for growing and dividing, when subjected to the action of stains, appear by no means all alike. Any one who has actually studied centrosomes will have a lively impres-

sion of the extreme difficulty with which they can be made to take a stain at all, and the frequency with which they remain invisible at the spindle apices, although there may be abundant stained microsomes elsewhere in the cell. It is thus evident that certain microsomes stain when the centrosomes do not, or, in other words, that the micro-chemical behaviour is not the same. When, however, the centrosomes do stain, it is equally true that there are usually seen numerous microsometa from which, but for their position, it would be impossible to distinguish them. Consequently, we are at present incompetent to say whether the centrosomes are what they are, by virtue of more than mere position, or not; because the solution of this difficulty lies beyond our present means of observation, and therefore cannot be determined. The only conclusion warranted by the facts is that the centrosomes do persist through successive generations in many forms of tissue cells.

On turning now to the few cases in which the spheres have already been found to exist in the Protozoa, it will be seen that the main features of the previous description, respecting the modification undergone by the metazoan spheres, are here repeated with a curious exactitude. In Euglypha alveolata, according to the beautiful figures of Schewiakoff (29), centrosomes appear at the apices of the division spindles, which are destitute of archoplasmic surroundings and at the same time intra-nuclear in position. Beyond the unruptured nuclear membrane, fans of radii spread through the protoplasm and are strictly comparable to those of the metazoan radial envelope. Owing to the intra-nuclear position of the centrosomes, the spindle would appear to originate from the nuclear substance, which probably here replaces in part the archoplasm.

In the cystoflagellate *Noctiluca* there is a large extra-nuclear and typically compound sphere, which is comparable in every detail, even down to the little light space surrounding the centrosomes, with the compound spheres of the metazoa. When the centrosomes divide and the central spindle is formed, it lies in a groove in the nuclear membrane, exactly similar to that seen in the embryonic

genital cells of the salamander. The chromosomes are attached to the fibres of the radial envelope, while the Verbindungsfäden (*i.e.*, the fibres in the equatorial region of the spindle after the chromosomes have separated) are produced, according to Ischikawa, by strands of linin becoming stretched out between the separating chromosomes (Fig. VI.).

The sphere is in *Noctiluca* singularly well adapted for the exhibition of the concentric zoning of its structure, even its external envelope being sometimes figured by Ischikawa with a sharp outside boundary, beyond which there stretch the less definite protoplasmic contents of the cystoflagellate's body. I have myself observed in the embryonic genital cells of the salamander a similar sharp boundary to the outer surface of the radial envelope of the sphere, and when we bear in mind that the resting centrosomes, as Heidenhain insists again and again, are to be regarded as the middle point of the whole cell, it is quite possible that the concentric zoning may have some relation to osmotic influences operating on the inner structures from without.

However this may be, the arrangement of the parts of the sphere in diatoms is strangely different from the above. As described by Lauterborn (24) the sphere in these monoplastids is represented at the commencement of mitosis by a small *globe* and a *rod*, which lie outside the nucleus and are quite separate from one another. The globe is the centrosome, and from it stretch radiations comparable to those of the radial envelope of a compound, or the single radial zone of a simple sphere. The rod swells up into a spindle, round which the chromosomes become normally grouped, while the centrosome, dividing, passes to each end of the spindle figure. It will thus be seen that the sphere of a diatom is in the same dismembered condition as the sphere I have described in the spermatocyte of a rat, and that thus the monoplastid analogue of the most curious modification in the polyplastid sphere which I have come across is found among the diatom, one of the most aberrant forms of all unicellular organisms.

Up to the present time, the whole cell has been usually

taken as the morphological unit, but this idea does not apply to morphological conceptions of structures which are themselves intra-cellular in nature. The only units of which we can avail ourselves in such cases are constituted by the microsomes and the more or less definite fluid reticulæ in which they are suspended. In the individual cell, such units are always arranged in an organised complexity, of which the apparent differentiation into nuclear and cytoplasmic structures is the visible expression. And we have seen further that, beyond this structural differentiation, any superadded complexities of parts are always evolved during the course of cellular ontogeny. Nevertheless, these superadded structures are often purposive, and consequently ought to be regarded as organs of the cell; and it is one of their most remarkable features that such similar and obviously homologous cellular organs are apparently not always evolved from antecedent structures of a similar kind.

We have seen that the spheres in the metazoa are apparently modified in a limited number of ways, and although each type of sphere seems to be restricted to similar kinds of cells in different kinds of animals, concerning the origin or significance of these modifications hardly anything is at present understood.

The ascertained existence of attraction-spheres in relation to the division of the Protozoa is in itself a fact of great importance in comparative morphology, because it banishes once for all any doubt as to the identity of the protozoan body and the metazoan cell; while the curious similarity in the modifications of the proto- and meta-zoan spheres, to which I have herein drawn attention (*supra*), is a fact of which it is at present impossible to estimate the full significance. It is, however, certain that the dismembered condition of the sphere in diatoms, and its correspondence with a similar condition of the sphere in mammals, are facts of more than merely passing interest; and yet they are in no way more remarkable than the correspondence between the other types of proto- and meta-zoan spheres. Few things, one would suppose, could be further apart in the organic series than a *Noctiluca* and the

spermatocyte of a Salamander, yet they both present an identical modification in the parts of their spheres. Can it also be mere coincidence that these organs have been modified along identical lines in things as widely separated as a Euglypha and the spermatocyte of an Ascaris? I conceive rather that these facts must foreshadow the appreciation of morphological generalisations, which will embrace such apparently capricious yet similar variation in the spheres at the extreme ends of the organic series, and that it is among the prime objects of progressive cytology to ascertain what these "laws" may be.

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KEW THERMOMETERS—A CORRECTION.

IN an article on the "Measurement of Temperature" in the September number of "SCIENCE PROGRESS," p. 69, I wrote as follows: "I am afraid that the issuing of those Kew certificates which give the second decimal figure of the correction, so far from being a prevention, is a cause of inaccuracy, as, unless used with a full knowledge of the variations consequent on changes in the conditions, they impart a false confidence to their possessors".

I have received from Mr. Chree information which leads me to the conclusion that I was in error in assuming that the department at Kew issued certificates of the nature indicated. There have passed through my hands altogether three "lists of observations" giving the results of a comparison of certain thermometers with the Kew standard to within ·01°, and I was under the impression that such lists were regarded as certificates; and further, that their acquisition depended solely on the payment of an additional fee.

Mr. Chree points out: (1) that such "lists of observations" are not certificates; (2) that they were only issued under exceptional circumstances to scientific men of well-known standing. He also states that only very rarely, and also under exceptional circumstances, are certificates issued giving the correction to ·05°C.

I regret if my own misapprehension in this matter should have in any way influenced the judgment of others, and I am glad to learn that the authorities at Kew are more cautious than I had supposed.

I cannot, however, refrain from pointing out that their reservations in this matter lend additional weight to the opinions I expressed (in the paper above referred to) as to the difficulty of accurately measuring temperatures by means of mercury thermometers.

Finally, I consider that I have done the department a

service in warning the public against the misapplication of Kew certificates, for instances abound where observations of temperature are given to the $\frac{1}{100}$, nay, the $\frac{1}{1000}$, of a degree by observers who have based their observations on a thermometer standardised at Kew.

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